ETC

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an international quarterly of discussion, research, and theoretical inquiry in Non-Aristotelian systems and General Semantics

in Volume VIII, Number 2:

WELLER EMBLER discusses the unconscious influence of metaphor in determining social attitudes and beliefs.

Can sociology be made mathematical? NICOLAS RASHEVSKY describes new horizons in mathematical biology and sociology.

Science and values are not separate in the new discipline of Operational Anthropology, declares LAURA THOMPSON.

RICHARD D. LOEWENBERG explores the frustrations of children, immigrants, and the mentally ill in trying to be understood.

Grodzins' 'Americans Betrayed' is reviewed by WILLIAM R. CATTON, JR.; Rapoport's 'Science and the Goals of Man' by CHARLES F. HOCKETT.

etc. etc.

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ETC.: A REVIEW OF GENERAL SEMANTICS

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METAPHOR AND SOCIAL BELIEF

WELLER EMBLER *

GRAMMARIANS have often busied themselves defining what a metaphor *is*. But it is more meaningful in our day to find out what a metaphor *does*. The little words 'like' and 'as' exert an enormous influence over our thoughts and our behavior; and there is vastly more to figurative language than the customary pedagogical distinctions between similes and metaphors. Our behavior is a function of the words we use. More often than not, our thoughts do not select the words we use; instead, words determine the thoughts we have. We can say with some assurance that language develops out of social conditions and in turn influences social behavior.

Modern rhetorics and grammar books insist that written and spoken English shall avoid the hackneyed figure of speech. Triteness, they say, is 'evidence of a failure to attain animation and originality in expression.' But the trite figure is worn-out not because it has been used often before, but because it cannot bear the burden of new attitudes. Consider the following extended figure of speech from the novel *Young Man with a Horn* by Dorothy Baker.

> Fortune, in its workings, has something in common with a slot-machine. There are those who can bait it forever and never get more than an odd assortment of lemons for their pains; but once in a while there will come a man for whom all the grooves will line up, and when that happens there's no end to the showering down.

Fortune (or the more common 'Fate') is a concept for which the only referent in the external world is a series of observable events without an assignable cause, and creative writers in all ages have sought analogies which will force the concept of fate, for the time being, at least, to accept a local habitation and a name. For instance, to the Elizabethans, it was quite believable that the capricious order of happenings should be governed by malicious creatures who looked like very old women. When the fantastical Witches appear before Macbeth and Banquo announcing Macbeth's ascendancy from Glamis to Cawdor to King, it appeared to Shakespeare's audience that the course of Macbeth's life was chargeable to the whims of the horrid sisters. What is important is not the originality of the metaphors 'invented' by Dorothy Baker and William Shakes-

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peare but the relationship which their figures of speech bear to their times. If we think of events of our lives as controlled by witches or even controlled by some force known only to witches, we shall behave in one way; if we think that the course of our experience is a matter of statistical probabilities, we shall behave in quite another way.

A WHOLE philosophy of life is often implicit in the metaphors of creative writers, the philosophy of an entire generation, indeed, even of an entire civilization. In the great tradition of the western world, it has been common to liken (in some essential respects) men to gods. Classical man so loved the gods he had created that he wished to emulate them and to be like them. Ulysses was not a god, but he had many things in common with the classical divinities, and it was fair to call him god-like. In other words, it was important to the social attitudes of Homer's Greece to believe that some men at least were 'like' gods; and it is common knowledge that this belief in the divinity of man was responsible for much that was fairest and best in the classical civilization. The statues of Phidias are metaphors inspired by this ideal; the temples and the great dramas and the noble philosophies all testify to the comparison.

By way of contrast to the Greek, consider the figurative language of a modern novelist. In one of John Steinbeck's short stories, 'The Leader of the People,' the old grandfather of the story had at one time been a man of the frontier world of Indians and buffalo. But it wasn't Indians that were important, he tells his grandson, Jody, 'nor adventures, nor even getting out here. It was a whole bunch of people made into one big crawling beast. . . . Every man wanted something for himself, but the big beast that was all of them wanted only westering.' And while Jody thinks of 'the wide plains and of the wagons moving across like centipedes,' his grandfather continues, saying 'We carried life out here and set it down the way those ants carry eggs. And I was the leader.'

We must remember that Steinbeck matured as a writer during the depression era and that his social philosophy grew out of the social problems of the 1930's. In his search for a social philosophy which could meet the problems of his day, he turned for assistance to the biological sciences. In these he found sound method, tested hypotheses, and, if it could be translated into language descriptive of human behavior, a body of usable information about subhuman life. Steinbeck was one of the first American novelists to think consistently and seriously, but not always clearly, and mostly with a political purpose in mind, of men as something other than men. And it became Steinbeck's habit to compare human beings with marine animals, with land animals, and with insects. It may be fairly said that Steinbeck's dramatic similarities between mice and men, between fish and men (*Sea of Cortez*), between centipedes and men, whether drawn from observation or embedded within the firm system of ecology, have changed the social thinking of many readers.

TO FUTURE generations, an age may be known by the metaphors it chose to express its ideals. Between 1798 and 1859 a good deal happened to change men's minds about the world they lived in. Among other revolutions in thought not the least effective was the change in attitude toward nature. Wordsworth had said that nature was full of consolation, of joy, and of wisdom. Presently, however, as a result of geological and biological investigations, nature ceased to be regarded as 'Wordsworthian' and came to be thought of as 'Darwinian.' The theory of natural selection brought about a new attitude toward nature that had perforce to be expressed and communicated in new figures of speech. Tennyson was not simply striving to attain animation and originality in expression when he described nature as 'red in tooth and claw.' The association of abstract nature with tigers was striking, but for the Victorians it was also to become 'true.' *In Memoriam* anticipated the *Origin of Species* by nearly a decade, but its representation of nature as a tiger was subsequently to assist in the firm entrenchment of the Darwinian hypothesis; in fact, I suspect that it did more to consolidate the philosophy of struggle than did the *Origin of Species* itself.

As a force behind nineteenth-century socialism, Edward Bellamy's famous stagecoach metaphor in his *Looking Backward* had an effect that was immense. Its pertinence to the social conditions of the time, and the 'rightness' of its phrasing gave verbal form to many an inarticulated thought. The analogy of the watch in eighteenth-century Deism was so befitting the ideas of the scientists, poets, and philosophers of the age that the analogy became a 'truth' and God a cosmic clock-maker. When an age abandons its attitudes, it will exile its figures to the limbo for clichés, and new metaphors will take the place of the old. T. S. Eliot inspired a whole generation with the image of the wasteland; and in 1935 Horace Gregory's poetic picture of a chorus of men joining hands to make a new world supplanted, for a time, the figure of the wasteland.

Metaphor as Statement of Fact

BUT IT IS HERE that we must observe an important linguistic and social phenomenon. It will be noted that Homer does not say that men *are* gods but only that in certain respects they resemble gods. Wordsworth does not say that nature *is* a teacher, but only that nature is like a teacher. Yet when metaphor is new, those who find their attitudes implicit in the metaphor construe the metaphor to be a statement of identity, that is, a statement of fact. Figures of speech, when they are fitting and felicitous, and especially when they occur in print, give poetic sanction, as it were, to hitherto dimly felt, inarticulated beliefs. When metaphor is new, and when the reader does not enjoy the perspective vouchsafed by time, the metaphor is taken literally, and its function is not that of rhetorical device, but of statement of fact, *prescribing* certain kinds of be-

havior. *Indeed, it may be said that the habit which sees the germane metaphor as a statement of identity is a habit which changes the character of civilizations.*

Iago is a villain, but a clever one, and he says many things which were pleasing to an Elizabethan ear. For instance:

> 'Tis in ourselves that we are thus or thus. Our bodies are our gardens, to which our wills are gardeners; so that if we will plant nettles, or sow lettuce, set hyssop and weed up thyme, supply it with one gender of herbs, or district it with many, either to have it sterile with idleness, or manured with industry, why, the power and corrigible authority of this lies in our wills.

We may find Shakespeare's figure entertainingly turned, and we may even agree with the sentiment expressed, but in the twentieth century we shall hardly be aware of more than a casual similarity between ourselves and gardens. But when my friend explains to me that life is like a pin-ball machine — that we are little balls shot out through an alley, kicked around from place to place, sometimes ringing a bell or flashing a light, and eventually falling into a trough and rolling out of sight — when he says this, I inquire whether his figure is not perhaps the perfect analogy. His figure has a lively meaning for us today because we believe in the laws of probability, and half suspect the aimlessness of our existence. For all practical purposes, I am well prepared to think of life and pin-ball machines as identities, of taking my friend's metaphor as a statement of fact about human experience.

When we read in Friedrich von Schelling that 'architecture is frozen music,' we are charmed with the originality and neatness of the analogy. But when James Johnson Sweeney in describing Piet Mondrian's painting *Victory Boogie-Woogie* says that 'The eye is led from one group of color notes to another at varying speeds; at the same time contrasted with this endless change in the minor motives we have a constant repetition of the right angle theme like a persistent bass chord sounding through a sprinkle of running arpeggios and grace notes from the treble,' we are convinced for the moment that the canvas *is* music.

The figure of Socrates as a gadfly is interesting but seldom taken literally. Yet when the villain in the cinema is called a 'rat' and shot thrice in the abdomen forthwith, it is a nice question whether the villain was a man or a rat. We know that Grieg's music is not pink bonbons stuffed with snow, although we rather admire the aptness of the similarity and the sensuousness of the image. Yet when someone says of the music of Shostakovich that it is the 'cacophonous scream of a communist manifesto,' we wonder if that is not perhaps 'true.' We are delighted with the little girl who said that carbonated water tastes 'like my foot's asleep,' but when we say a dish we don't like is 'garbage,' it is indeed! When Byron says of poetry that it is the lava of the imagination which prevents an eruption, the metaphor strikes us as an amusing romantic definition of

poetry; but when a contemporary writer (I have forgotten who) says that a poem is the sublimation of the social irritant which troubled the poet, just as a pearl is the 'higher order' of the speck which irritated the oyster, we accept the metaphor as a statement of fact about all poetry.

The intricate and highly complex network making up the human nervous system has often been likened to a telephonic organization in which messages are sent out from central offices over the 'wires' of the system to all areas of the organism. The analogy is a picturesque explanation of the way the human nervous system 'seems to work.' At the moment, the telephone metaphor is being supplanted by an electronic metaphor in a new system of efficiency called Cybernetics. In this system the mind is likened to a communication system which has its own 'feedback,' making possible automatic adjustments between what it imagines to be 'out there' and what is really out there. But what the human nervous system will be likened to a century from now is unpredictable. New analogies will depend on future neurological theories and on future social, moral, and technological developments.

Most figurative language is not dangerous in its effects. It is when we take political and social metaphors literally, as statements of identities, that we must proceed with caution. Far more than we are aware, the way we use language determines what the social philosophy of our society shall be. When we take figurative language literally, we are in danger of behaving as if something were true which is manifestly not true *unless we proceed to make it so.* Orators disturbed with social conditions will see new relationships and will express them with vigor. The figure of the 'brotherhood of man' arose out of the way the Age of Reason chose to look at its political situation; the figure was unusually far-reaching in its political effect.

Effect of Metaphor on Modern Behavior

LET US EXAMINE several current metaphors, stated or felt, which express 'truths' of modern social beliefs and which, because they are often taken as identities, are in many ways responsible for modern social behavior.

1. Murray Schumach's extended figure of speech which appeared on the front page of The New York *Times* for February 26, 1946, is, to be sure, innocuous enough; moreover, it is an analogy which occurs to us as rather common, one which would seem to most people living in New York City to bear itself out in many exact ways:

> Complete shutdown of the city's subways would be almost as disastrous to the city as a serious circulatory ailment to a human being. For New York's subways are her main veins and arteries, distributing her human corpuscles throughout the municipal body twenty-four hours a day.

Mr. Schumach had no ulterior motive, I am sure, in describing New York City in this fashion. I do not want to be misunderstood in what follows as saying that the authors of social metaphors are each and all conspirators against the public morale. Nor do I suggest that Mr. Schumach has caused the inhabitants of New York City to think of themselves as corpuscles. The idea had probably occurred to some people before. But I do suggest that the author of this figure has himself seen New York City in this way and that henceforth some of his readers will unconsciously identify *other* people in New York City as corpuscles. The effect of such metaphors is subtle. If the city is personified as a monstrous human being, and its inhabitants think of themselves as red and white corpuscles, presently, from their understanding of what red and white corpuscles do, they may be inclined to act as if they were, and that might or might not be a good thing; but if carried to extremes, the habit of behaving like a red corpuscle within a monstrous body will change the culture patterns of a society.

2. If it were not for the fact that a colleague of mine brought the following parallel to my attention, as a parallel that came spontaneously to his mind, I should consider it rather too far-fetched for inclusion in this essay. In the December 28, 1942 issue of *Life,* there is printed an extraordinary picture of Field Marshal Rommel's army retreating westward along the Mediterranean coast. The picture was photographed from a British plane, and it strongly resembles a slide of bacteria as seen under a microscope. New instruments, new inventions cause us to 'see' things differently. In the *Life* picture we see thousands of men who look 'exactly' *like* bacteria. The resemblance is so striking that for a moment we suspect an identity. If we say enough times that men are like or look like bacteria, and if we see enough pictures showing the resemblance, we shall soon begin to behave toward men as we behave toward bacteria. (One is reminded here, for example, of the rather widespread use of the phrase 'to be immunized against totalitarian philosophies.') Most important of all, from the social point of view, is the fact that our world is prepared in a great variety of ways to think of men as bacteria. Was it Robert Louis Stevenson who referred to humanity as a disease on the face of the earth? In any event, more than one sensitive person has taken Stevenson's metaphor literally and despaired.

3. I have often heard large cities referred to as jungles. The main characteristics of a jungle in darkest Africa, as I have read, are an amoral attitude toward all creatures outside the family, the need for alertness in order to survive, the constant feeling of insecurity, and the value of certain traits such as strength, cunning, and agility. Now it is quite possible that these are characteristics of modern city life, and in these respects, therefore, life in a city is like life in a jungle. This is, if true, and we suspect it is, unfortunate, for no one whom I

know wants to live in a jungle. It is perfectly natural to think of a city as a jungle, perhaps, but the more we think of it in that way and the more we extend the analogy, the more likely it is that the city will *be* a jungle. At the moment, at any rate, a large city is not a 'jungle,' it is only in some respects like a jungle, and presumably the differences can be pointed out. When, therefore, we insist that the city *is* a jungle, we are no longer speaking figuratively, we are making a statement of moral fact; indeed, we are issuing (as did Upton Sinclair) a moral ultimatum.

4. Oswald Spengler was a master of intellectual analogy, perhaps the greatest of all time, certainly second to none in the modern world. *The Decline of the West* foretells the eventual demise of the western world, but the entire structure of Spengler's thesis rests on a metaphor. The organic theory of history is a figure of speech, which, simply put, says that a civilization is *like* a living creature — it is born, it grows, and it decays. But Spengler's book is no cause for pessimism, for a civilization is not identical with a living being, as will be seen when Spengler is as old as Homer. A civilization seems to be in some (pertinent, we admit) respects like a living being. But it should have occurred to someone long ago, and perhaps it did, that to take Spengler literally is to put all one's faith in the logic of 'if.' If one believes that the western world has entered upon a stage of decline, then what Spengler says about the western world is 'true.' If there were to take place an historical experience such as a 'decline,' it would perhaps happen as Spengler has described it. In fact, so long as we understand Spengler's analogies to be identities, so long shall we believe in, *and perhaps assist in,* the inevitable decline of the western world.[1]

5. One of the most pertinent of modern metaphors, one that seems to many young people to be an accurate description of our modern world is the analogy of the myth of Sisyphus with contemporary experience. Sisyphus was the mortal condemned by the gods for his sins to roll a stone to the top of a mountain from which the stone falls back down the mountain of its own weight, and Sisyphus must again roll it to the top. The labor of Sisyphus goes on endlessly forever. Albert Camus, contemporary French novelist, and expositor of the 'philosophy of the absurd,' has written a philosophical work entitled *The Myth of Sisyphus,* and in this work he says, 'The gods thought with some reason that there was no punishment more awful than useless and hopeless labor.' Although Camus thinks of Sisyphus as a tragic hero, noble in his scorn of the gods, he also thinks of him as the absurd hero, and he likens the worker of

[1] Talking about a 'possible' future event will in itself tend to bring the event to pass. And the evidence selected to anticipate the event will direct the event to take the form foreseen. That people often behave according to the way they have expressed their expectations is brilliantly discussed by Robert K. Merton in his article 'The Self-Fulfilling Prophecy,' *Antioch Review,* 8.193ff (1948).

today to Sisyphus. 'The worker of today labors every day of his life at the same tasks and his destiny is no less absurd.'

But the myth of Sisyphus is a story, as a moving picture is a story, as a ballad is a story as an epic or a novel is a story. We often think of ourselves as 'like' a character out of a novel or story. In fact, a whole nation of people (generally speaking) thought of themselves as 'like' the Siegfried or Brunhild of Teutonic mythology. It is pleasant to some people to suspect that they have something in common with Siegfried, but if we say we *are* Siegfried, unless the whole world agrees with us, we are in trouble. We are not less in error if we take the myth of Sisyphus literally and say that we are Sisyphus, that we are absurd heroes engaged in 'useless and hopelesss labor.' It is only too patently true that our world seems to be 'like' the Hell of Sisyphus. But that our world *is* the Hell of Sisyphus and that we *are* doomed to hopeless labor is a fiction evolving out of an inferred identity which simply does not exist. Camus has used his parallel as a device for illuminating and criticizing the social world of today. If we take his figure literally, and especially if we take it without an understanding of its 'tragic' implications, we are likely to be very unhappy if nothing more.

6. A good deal of theoretical discourse is diagrammatic in character. The Ptolemaic cosmology in the form of a diagram says only that as far as can be observed and inferred, the universe is 'like' the diagram. Diagrammatic representations which are the results of inference and imaginative picturing forth of an idea cannot be tested empirically, and we must beware of seeing identities where they do not exist.[2]

To take the most illustrious modern example, I turn to the popularized psychology of Sigmund Freud. (But first I should like to point out that I have the greatest respect for Freud, and I should be the last to choose to discredit psychoanalysis. Only provincial prejudice finds Freud ridiculous and offensive.) Freud was a poet and a philosopher. Freud's work is one long extended figure of speech, as is the *Divine Comedy* of Dante. Both Dante and Freud have written allegories of the spiritual life. If there were a Hell, it might be 'like' Dante's. I think it was Waldo Frank who said somewhere that if you believe in Hell, it makes a great deal of difference to you how may circles there 'really are.' If you take Freud literally, the true nature and character of 'complexes' makes a great deal of difference; but what the analyst says in effect is that we act 'as if' we had an Oedipus complex, supposing there were such a thing as an

[2] For an analysis of schematic, symbolic, and tropic 'fictions' see Hans Vaihinger, *The Philosophy of 'As if'* (London 1949), 24ff. In fact, Vaihinger's work is throughout a painstaking study of the consciously false assumptions which are implicit in most if not all metaphorical expressions. In many respects, my 'Metaphor and Social Belief' is a kind of extended footnote to *The Philosophy of 'As if.'*

Oedipus complex, just as a physician might have said in the seventeenth century that his patient behaved as if he were a witch, supposing that there were such things as witches. But the 'cure' for witches was different.

Freud was a philosopher whom many people have taken literally, assuming that the subconscious as Freud describes it is *identical* with some 'part' of our 'self.' Freud's diagram of the Id, the Ego, and the Super-Ego is a schema which is metaphor useful merely in discussion. In *An Autobiographical Study* Freud warned against taking his schematic representations literally. 'The subdivision of the unconscious is part of an attempt to picture the apparatus of the mind as being built up of a number of *functional systems* whose interrelations may be expressed in spatial terms, without reference, of course, to the actual anatomy of the brain.' But many readers will take the image of the subconscious literally. We imagine the Id as a murky 'depth' of turbulence somewhere at the bottom of the 'mind,' its poisonous fumes rising up from deep upon deep of mystery through the trap door of the censor at night when the censor is 'asleep.' The Ego, our every-day consciousness, lives on the first-floor of the mind, looks out of its windows, and conforms to the customs of the every-day world—the bewildered little man of the cartoon who always stands between two opposing forces. Above, at the 'top' of our mind, is the Super-Ego, a kind of stern father who looks down upon our conscious behavior and issues commands.

Even professional analysts sometimes 'think' in this manner; and amateur psychologists, as I have discovered, almost invariably do. It is impossible to know how much such a 'metaphor' has affected the common reader in his attitudes toward life, but it is fairly certain that many have taken the figure of the subconscious literally, saying that this is the way the mind works. On the contrary, we should say that our inner life seems only in some respects to be *like* the Freudian description.

7. Drawing analogies between the subject-matter of zoology and biology and the subject-matter of the social sciences has been very popular in recent years among some social thinkers. It has been fashionable to think of men as animals and even as insects. One of the most common parallels drawn by fascist theorizers is the parallel between men and ants. I recall magazine articles describing Nazi scientists observing ant societies in order to discover how human society may most easily be converted into a society similar to (or identical with) that of the white ant of Africa.

The propagandistic use of the ant metaphor has been most vividly described by a character in Arthur Koestler's novel *Arrival and Departure*. In the enthusiasm with which Koestler's Nazi speaks, we see among other things that the language he uses has convinced him beyond any doubt of the desirability of understanding that ants and men are, for political purposes, identical. When, as in Nazi Germany, men are likened to ants, then beyond question men will be

treated as though they were ants. Whatever is thinkable is also possible in the world of today.

> Absolute conditioning must finally result in the creation of a collective consciousness in the full biological sense of the term. Nature has a perfect working model for it in the city-states of the African white ant. They each embrace several million members of the race, they cover areas up to fifty square miles and function with absolute expediency. They have perfect division of labor; they control highly complicated technical devices including a system of heating by vegetable fermentation which keeps the temperature in their vaults constant through all seasons; they enforce a mathematically perfect birthrate policy. And yet they have no planning body, no blue-prints, no governing administration, not even the means of written communication. How is this possible? The answer usually given is 'by instinct'; but such a highly differentiated instinct which is shared by, and limited to, members of one city-state amounts to nothing less than a collective brain-function of the state organism. In a similar way, and probably helped by artificially-produced biological mutations, the individuals in the supra-state will become mere cells in an organism of a higher order—a million-legged, million-armed cyclopean colossus. . . .

8. It is apparent from the illustrations I have used that our contemporary social similitudes are often drawn from the biological sciences. The master metaphor of the modern world, however, is drawn from technology and is the identification of men with machines.

In our thinking we often, nay almost invariably, make the distinguished blunder of supposing that the human body is a machine, that it works as a machine works, according to the laws of mechanics. *Why* we prefer to 'think' in this way about ourselves is open to speculation. In any event, that we should be unaware of the simple fact that men are only in a few unimportant respects like machines, but that machines are, *as far as they go,* in all respects like men is the supreme misunderstanding of our time and accounts almost exclusively for our lack, just when we need it most, of a real humanistic philosophy. The obvious fact is that machines are like men for the simple reason that man made the machine, as far as he could, *in his own image.* It would be unpleasing to some ears to have it said that man left out a soul when he made the machine, but that, or something very like that, is precisely what happened.

In recent years the machine has been perfected to perform uncanny tasks, the most striking being the calculation and solution of involved mathematical problems. The electronic brain has its place in the imaginations of men as the Most High. Presently we shall wish that our nerve cells were as clever as electron tubes, quite forgetting that it was our nerve cells that had the wit to create electron tubes.

Make no mistake about it, we think more highly of our machines than we do of ourselves. We try to live up to the machine, to learn from the machine, to be

more nearly machines, because we think they are better than we are.[3] It appears that our dearest wish is to *be* machines and to function wholly as they do. This wish will strike future ages as the dominating feature of a lost society, of a deluded people who worshipped their engines more than their gods, to say nothing of themselves.

But creative writers and some social philosophers have been saying all this for more than a hundred years. There is little excuse for me to labor the point. All I wish to insist upon is that the way we use language may account in part for our madness. The moment we say that the human being *is* a machine, at that moment we shall believe that the human being can be conditioned to behave in a perfectly predictable way with admirable regularity. Then it is that we shall have made of ourselves something less, ever so much less, than we are. But if we speak with caution to the effect that machines are *in some respects* like human beings, we can put the machine to its good use and still keep our self-respect and our humanity.

IF EVER we needed to think of man as man, as unique, identical with nothing else in the world, it is today. Contemporary imagery is blurred and confused. Our hardest intellectual task is to keep things apart, to separate out issues and ideas from one another. Our present efforts are to see all particulars as alike, and we tend to force identities where only similarities exist. No two things are identical, although we seem to want to think so. We have abandoned the exact and differentiated in favor of mingled associations and blurred relationships. The use of analogy in all branches of human thought is indispensable; but the making of nice distinctions is equally important and has ever been a leading characteristic of the human mind. The knowledge of subtle differences—as between people of different cultures, between one human being and another, or even between brands of coffee, kinds of perfumes, vintages in wines, spoiled food and edible food, poison and medicine, beauty and ugliness, justice and injustice—is a knowledge invaluable to mankind. What is more, the knowledge of differences leads to an understanding of relationships. We are more likely to be at home in our world not so much by striving to cut each particular to the same pattern, as by studying to understand the tenuous, the subtle, the often beautiful relationships which are the foundations of human society and to a great extent the joy of life. The knowledge of just and workable relationships among dissimilars is what every philosopher, every artist, and every statesman has longed to attain.

[3] While preparing this article for publication, the writer was introduced to *Dianetics* by L. Ronald Hubbard (New York 1950). In this 'modern science of mental health' would appear to be the most appalling example yet of the identification of men with machines. Constructed in the name of efficiency, the central theme of *Dianetics* depends on the fiction that a human being is a calculating machine capable of functioning in a 100 per cent mechanical fashion.

FROM MATHEMATICAL BIOLOGY TO MATHEMATICAL SOCIOLOGY

NICOLAS RASHEVSKY

[FOREWORD: *The fruitfulness of extending physico-mathematical methods of inquiry beyond their traditional applications in the physical sciences has been a principal tenet of general semantics. Nicolas Rashevsky, chairman of the Committee on Mathematical Biology at the University of Chicago, has been a foremost pioneer in carrying out this program. Under his leadership a group of scientists at the University of Chicago have been mathematicizing biology for the past fifteen years, and it was inevitable that the sheer momentum of their progress should have carried them to the first formulations of mathematical social science.*

The method of mathematical biology and mathematical sociology can best be described in general semantics terms as 'consciousness of abstracting.' To quote Rashevsky, 'No postulate ever corresponds to any "ultimite reality." ' Anyone who has been to a single introductory lecture on general semantics will immediately recognize this negative assertion as the first non-aristotelian principle formulated by Korzybski as the principle of non-identity. The recognition of non-identity of maps and territories both gives the mathematical biologist freedom to perform 'mental experiments' with innumerable models of living systems and keeps him constantly aware of the non-conclusiveness of even the most spectacular predictions based on these models. In giving up hope of ever describing 'ultimate reality' (as the physicists have done), the mathematical biologist and sociologist wins the hope of steadily approaching a formulation isomorphic (similar in structure) with 'reality,' the closeness of fit being operationally defined in terms of prediction value.

Another field in which Rashevsky and his colleagues are actively putting into effect the Korzybskian program is the mathematical theory of the central nervous system. Where Korzybski spoke necessarily in vague terms of multi-ordinal structure and events on the colloidal level (vague because knowledge of such structure and such events were not available in 1933), Rashevsky and his colleagues have defined certain aspects of the structure of the nervous system mathematically. An impressive body of theory is arising which will give impetus to empirical investigations seeking to relate the structure of our nervous systems with our evaluative processes. The framework of a bridge from the physical to the biological to the social sciences is thus becoming discernible.

Dr. Rashevsky's paper was presented at the Symposium on Scientific Method at Roosevelt College on October 20, 1950. THE EDITORS.]

WE ARE assembled here today to discuss the question: 'What is scientific method?' Representatives of the physical, the biological and the social sciences will discuss the question from their respective points of view. The three fields of knowledge are not independent and not entirely separate from each other. I think, however, that it will be generally agreed that there is a closer relationship between biology and the physical sciences on the one hand, and biology and the social sciences on the other, than between the physical sciences and the social sciences. Biology takes a great deal from physics and chemistry. The highly important researches in the borderline fields, biophysics and biochemistry, prove this assertion. On the other hand, biology gives a great deal to the social sciences. Problems of behavior, the study of which is one of the objectives of biological sciences, are of primordial importance in social sciences. Social psychology deals with both psychobiological and social problems.

The answer to the question: 'What is scientific method?' is clearly a definition. There are several ways of defining an object. Either we describe it in terms of other known concepts, or we merely point out the object to be defined and say: 'This is what we mean by such and such a word,' or, finally, we may give what is known as an operational definition, by prescribing the set of operations necessary to obtain the object under definition. In the present case the last two ways of defining become practically identical. It must be, however, emphasized that no matter what kind of definition we use, it remains always, like any definition, quite arbitrary. You may propose one definition, I may propose an entirely different one. The problem before us may then be either to agree on one definition or to propose as many definitions as possible, and then to discuss the various merits and shortcomings of the methods thus defined. While agreement is, in general, a very good thing, I am not sure at all that in the present instance a friendly disagreement may not be a better thing. It may be that if each one tries out his own method we will have in the long run more progress than if we blindly follow only one path which has been generally agreed upon.

In order not to lose ourselves in too much abstract philosophical discussion, I propose to limit myself to a much more modest task. I shall discuss the following, rather restricted, question: 'What methods have been used in what is generally accepted as science, by men accepted as scientists, and have led to unquestionably important results?' I am fully aware that by formulating the question in this way I am actually *avoiding* some difficult problems. Perhaps I am just paying a tribute to my natural laziness!

OF THE THREE fields represented at this symposium, the physical sciences have preoccupied mankind for by far the longest time. Compared to them, biology, all its recent progress notwithstanding, is a child. One of the reasons for this lies undoubtedly in the circumstance that no essential progress in biology

could have been made before certain developments in physics had taken place. It requires a microscope to establish the existence of cells, to discover and study microorganisms, to investigate the structure of tissues. And it requires not merely *a* microscope, but *a good* microscope. But a good microscope is the product of years of development of geometrical and physical optics, of mechanics, and of the chemistry and technology of optical glass. Similarly, present day studies in electrophysiology could not have taken place without the electronic tube, which is the product of years of research in electromagnetism. Even the purely descriptive systematic microbiologist or histologist depends in his work on the development of physics just as much as a physiologist or biophysicist does. In *this* sense we may say that Robert Koch and Fritz Schaudinn were no less biophysicists than are George Bishop or Kenneth Cole. It is not impossible that social sciences occupy with respect to biological sciences the same position as the latter occupies with respect to physical sciences. Perhaps a certain amount of development in biology is necessary before real progress in the social sciences can be made. We shall touch on that point later.

What method then did the scientists use through the ages of development of the oldest of scientific disciplines?

The word *science* comes from the latin *scio,* which means *I know.* Essentially, science means the same as knowledge. If we accept the French dictum: 'Savoir c'est prévoir,' 'To know is to foresee,' or, 'To know is to predict,' then the test of whether we know something is determined by whether we can predict. In natural sciences predictions have been numerous. How is a scientist able to make a prediction?

Schiller has answered this question as a poet, in a highly poetical way:

> Mit dem Genius steht die Natur im ewigen Bunde:
> Wass der Eine verspricht, leistet die Andre gewiss.[1]

Beautiful and inspiring as these lines are, they do not give an answer which can satisfy a scientist. To find a more satisfactory answer we must inquire into the course of development of a natural science.

EVERY science, except pure mathematics, in its early stages begins by an accumulation of factual knowledge through observation. From observation we pass to experiment, which is sometimes described as observation under controlled conditions. Though this description is essentially correct, it does not tell the whole story. The number of conditions which may be controlled in a given situation is frequently rather large. In attempting to keep some of the factors constant, the scientist usually bases the choice of those factors on some explicit or implicit assumptions, sometimes on simple, sometimes on very complex reason-

[1] 'An eternal pact binds Genius and Nature:
What the one promises, the other is sure to bring about.'

ing. In the simplest experiment the investigator merely asks the question: 'What is going to happen under certain conditions?' An experiment of that type was performed by Galileo when he dropped a bullet and a cannon ball from the leaning tower of Pisa, and found that they fell with the same speed, in spite of the differences in their weights. Since the bullet probably was made of lead, while the cannon ball was made of iron, the experiment showed also that the material of which the falling body was made did not affect its speed of falling. A similar experiment performed with a bullet and a feather would, however, clearly show that the bullet falls faster. Since the previous experiment indicated that the material is of no importance, the scientist looks for some other factor which may explain the different outcomes of the two experiments. He may *think* of air resistance as a possibility. But if we assume that air resistance is the factor which causes the feather to fall less rapidly than the bullet, then *it follows* that in vacuum both will fall with the same velocity. Here is a very elementary *theory,* consisting of an assumption and of a consequence derived from this assumption. To test it we perform the now well-known classroom experiment of having the feather and the bullet fall in vacuum. The result confirms the expectation of the 'theory.'

This example is highly oversimplified, but it illustrates the point. The design and planning of almost every experiment are the results of reasoning, based on implicit or explicit assumptions. The more complex the set of phenomena which we study, the more involved is the design of the experiment and the more complex also is the necessary reasoning. Beyond a certain degree of complexity ordinary logic becomes insufficient. The elaboration of the theory, the derivation of the consequences from the assumptions or hypotheses, requires the powerful tool of mathematical analysis. But in a rudimentary form the theoretical reasoning and an element of hypothesis are present in every experiment. Even in an experiment in which we merely ask: 'What is going to happen under such conditions?' there is a hypothesis involved, though this hypothesis becomes, in a sense, an accessory after the fact. When we find an outcome of an experiment we conclude that the same outcome will be found again and again under similar conditions. This is essentially a hypothesis which is justified actually by its predictive value. We assume a certain uniformity in nature. The law of induction is essentially an expression of this assumption, which turned out to be a very useful one. When we observe an outcome of an experiment and then say that this same outcome will be found if the experiment is repeated in the same way, we essentially make a prediction, based on our assumption of the uniformity of nature.

IT IS TRUE that this concept of uniformity has recently undergone a fundamental revision. From a strictly deterministic uniformity, we passed to a probabilistic, or statistical, uniformity. There is, however, a continuous graded

transition between the two. In the physical sciences the former prevails. That is why frequently only one or a few repetitions of an experiment are sufficient to establish some regularity or 'law.' In biology, and especially in the social sciences, statistical uniformity prevails. As Professor Wolfgang Köhler once said, we may pick up a leaf from the ground, determine very exactly its mass, its area, its color, its chemical composition, etc. We shall thus make a number of highly precise measurements. Yet the scientific value of such an 'experiment' would be nil. Why? Because there are no two leaves exactly alike, and we cannot therefore predict anything from our 'experiment.' But if we take one hundred leaves of the same species, and perform similar experiments on them, then not only can we predict that if we take another hundred such leaves the average results will be the same, but we can even predict the probability that a given leaf, picked up at random, will have given properties.

From what has been said it follows that any arguments as to whether the experiment or the theory plays a more important part has no meaning. No valuable experiment can be designed and performed without at least a rudimentary theory, but no theory has any value *per se* unless it can, at least in principle, be verified experimentally. Experiment and theory are two inseparable and interdependent parts of the same tool which the scientist uses in his work. True enough, the requirements of division of labor usually result in some scientists specializing in experimental work, others in theoretical. True also that in the design of experimental apparatus the experimenter may be temporarily involved in purely manual techniques which have nothing to do with a scientific theory. Similarly, in order to develop experimentally verifiable consequences of a theory a theoretician may have to indulge, as a necessary intermediate step, in some very abstruse speculations, which may have no relation whatsoever to any experiment. But in both these cases we have, as said above, temporary intermediate work. The close relationship between theory and experiment is not altered by this.

For some time the view was held by many scientists that the experimental verification of the conclusions drawn from a set of hypotheses constituted a proof of those hypotheses. The hypotheses were considered in such a case as actually describing *the* reality; they were considered as being 'true.' Since Henri Poincaré we know that this is not the case. There is no such thing as *the* hypothesis or *the* theory. There is rather an infinite number of hypotheses and theories which can explain a given set of phenomena. We do not speak any more of 'true' or 'false' theories, but of 'good' or 'bad' ones, or of 'convenient' or 'inconvenient' ones. A 'good' or 'convenient' theory is one which enables us to make the greatest number of predictions in the easiest way. The success and importance of the Relativity Theory are not due to the fact that the new concepts of space and time are more 'true' than the old ones. There is no physical phenomenon that could not be explained on the basis of absolute space and time.

But such explanations would, in most cases, be very cumbersome and artificial, while they become simple and natural on the basis of the Relativity Theory. That is what makes the theory a good one.

IN DEVELOPING a theory a scientist tries to construct a conceptual abstract system which is isomorphic to the world which he observes. A system completely isomorphic to the observable world would enable us to predict everything. So far the systems proposed have been only in part isomorphic to the observable world. Extension of this isomorphic region constitutes progress in science.

The situation may be illustrated by the following example. Imagine a geometric plane inhabited by two-dimensional intelligent beings who can move freely in that plane. As they move around they discover points in that plane. Their world is made up of those points. They find that with great approximation the first three points lie on a straight line. They then make the assumption that all points in their plane lie on this straight line. To test this assumption they perform an experiment by moving along the line, and they actually find a few new points. Having thus established a 'good' hypothesis they predict the existence of other points along that straight line, and their prediction is confirmed. Then someone discovers at a great distance a point which is decidedly not on the straight line, and then another such point. It is then found that all the points, including the newly discovered ones, actually lie on a parabola, and that the original hypothesis holds good only for a region of limited size, since any sufficiently small segment of a continuous curve is well approximated by a straight line. The old hypothesis is revised. A new theory is established, according to which the world consists of points lying on a parabola. This theory proves 'good' within a certain domain. Outside of this domain deviations are, however, found and the theory is revised again, on the assumption that all points lie on a higher order curve. But though an old theory is discarded, yet it remains valid within a limited region of the universe, or within a limited array of phenomena. Gradually the theory becomes extremely complex, as more and more points are discovered. In rectangular coordinates, which our imaginary beings are 'naturally' using, the equation of their 'world line' becomes quite unmanageable. Then along comes a 'mathematical genius' and he points out that everything becomes much simpler if we introduce appropriate curvilinear coordinates. In fact, in these coordinates the 'world curve' is represented by a linear equation, just as was the original straight line in the rectangular coordinates. The Relativity revolution in physics may be considered as just a change of the system of coordinates.

Thus we see that in formulating his hypotheses the scientist is not too much concerned with their correspondence to some ultimate reality. All he cares is that the theory developed on the basis of those hypotheses be isomorphic with the observed world. Even though a hypothesis is sometimes suggested by

an observation, it is *never* formulated so as to correspond exactly to the phenomenon which suggests it. Even the simplest phenomena are too complex to be grasped by the human mind in their entirety. The scientist picks out only a few salient features of the observed phenomenon, features in which he is interested at the moment. In other words, he abstracts from the phenomenon as a whole the features which are interesting to him. The whole dynamics of rigid bodies is based on the abstraction of a perfectly rigid body, which is nonexistent in nature. Yet, *within limits,* the equations derived on the basis of this abstract, utterly unrealistic, concept find practical applications in engineering!

AS WE HAVE said above, the study of the more complex phenomena of necessity leads to the use of mathematical analysis. One of the many advantages of the mathematical method is that it enables us to make *quantitative* predictions. We shall illustrate with three examples, one taken from physics and two from biology why a quantitative prediction is of considerably greater value than a qualitative one.

The development of classical electrodynamics was closely linked to the concept of the universal ether, the carrier of electromagnetic phenomena. From various considerations of a quantitative nature, such as the theory of astronomical aberration, it was concluded that the ether could not be carried along by bodies moving through it. From this one could conclude, *without any knowledge of mathematics,* that there should exist an 'ether wind' at the surface of the earth. Michelson would have devised an experiment to prove the existence of such an ether wind. Finding a negative result, what would his conclusion have been? Either that there is no 'ether wind,' or that if it exists it is too small to be detected by his experimental set up. The experiment with a negative result would have gone into oblivion with many other similar ones. Actually the situation was different. The *mathematically* elaborated classical electrodynamics did not merely require the existence of an 'ether wind,' but it required it to be of a definite magnitude. From the outset Michelson had to design his apparatus so as to be able to measure an effect of a *prescribed magnitude.* When he found no such effect, then there was no alibi that the effect might be too small to be detected. There was clearly a contradiction with the requirement of the theory, a contradiction which made a revision of the theory mandatory. The revision eventually led to the Theory of Relativity.

With a qualitative theory we may have 'negative experimental results.' With a quantitative theory the result of even a 'negative' experiment is positive.

BIOLOGISTS for a long time have been speculating on the possible causes of cell division. A rather appealing assumption was that it is due to electric charges. Cells were known to carry an electric charge. Charges of the same sign were known to repel one another. What was more natural than to assume that

electrically charged parts of the cell repel one another, causing the cell to divide. The very superficial resemblance of the spindle fiber figure to a drawing of 'lines of force' between electric charges of the same sign, as found in any physics textbook, further advanced the above-mentioned hypothesis. A great deal of rather elaborate *qualitative* speculation was made in this connection.

Only relatively recently, with the advent of mathematical biology, has mathematical analysis been applied to that problem. The magnitude of the electric charges in cells is approximately known. Hence we can *compute* the forces they will exert and the effects of those forces on the cell. The mechanical strength of cells has also been measured approximately. Hence it was possible to figure out mathematically whether the known electric charges in the cell could result in the division of the latter. The result was, in general, an emphatic *no*. With some exceptions the electric forces are considerably too small to produce cell division, though in some instances they might affect the course of the division.

But the further development of the mathematical theory of cell division did much more than merely yield the above result. In reviewing possible causes of cell division, it was found that one possibility is present always in any cell which manifests signs of life. That possibility is given by the forces produced by the diffusion of various metabolites into and out of the cell. A great deal of elaborate mathematical work had to be done to develop the theory based on this possibility. When it was done it was not only possible to show quantitatively that the diffusion forces are sufficient to produce cell division. It was also possible to calculate the average size of cells, and the result was found to be in quantitative agreement with actual observation. It was shown that cells with a higher glycolytic coefficient should divide more readily, which is generally the case. Finally, equations were derived which described quantitatively the process of division of an isolated spherical cell. The equation described the process of initial elongation, followed by a constriction at the equator. At the time the equations were developed by H. D. Landahl, no quantitative experimental data were available to verify them. Subsequently Dr. Ralph Buchsbaum, assisted by Robert Williamson, made numerous carefully controlled experiments on demembrenated Arbacia eggs. A typical result of comparison of the theory with their experimental findings is shown on the graph (Figure 1). The theory not only explained already known facts, it predicted quantitatively the results of a quantitative experiment.

A FURTHER development of the theory of 'metabolic diffusion forces' led to applications to some embryological phenomena. In particular, a theory of gastrulation was developed. In developing this theory, two possibilities appeared particularly interesting. One was that the cells of the blastula just before gastrulation were subject only to forces resulting from the metabolism of the

other cells. The other was that each cell exhibits a certain inner 'diffusion polarity' which makes for additional forces. The two possibilities lead to different equations for the rate of gastrulation. The second graph (Figure 2) shows the two curves, represented by the two equations in their comparison with some observations. There is no question but that the assumption of polarized cells leads to a better agreement with observations. If we had merely a qualitative theory, a discrimination between the two cases would have been impossible.

Individual instances of application of mathematical methods to biological problems date as far back as the end of the last century and the beginning of this century. Mention should be made of the work of J. L. Hoorweg, Weiss and W. Nernst on the theory of nervous excitation and of the very important work of O. Fischer on the dynamics of human extremities. The systematic de-

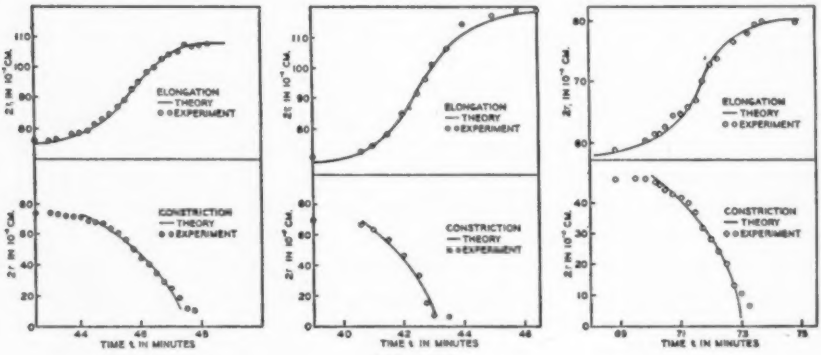

FIGURE 1

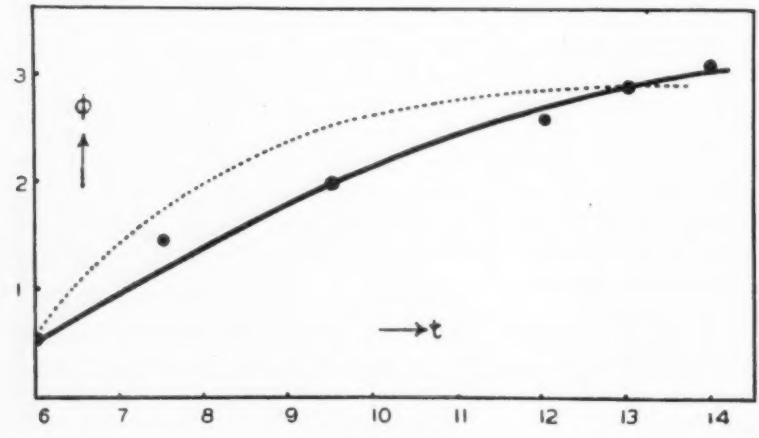

FIGURE 2

velopment of mathematical biology is only about a quarter of a century old, but it has yielded important results. We have mentioned already two examples, chosen from a large number of cases. Important contributions have been made by the pioneering work of Alfred Lotka and of Vito Volterra. They have developed a mathematical theory of the struggle for existence, a theory which leads to definite quantitative predictions: Volterra has shown that the interaction of two species, one of which preys on the other, results, under certain conditions, in periodical fluctuations in the number of individuals comprising the two species. Periodic fluctuations of the populations of some animal species, such as fishes and insects, have long been observed. A quantitative experiment to check Volterra's theory was first performed by F. Gause, who used two species of microorganisms. Periodical fluctuations were actually found. A quantitative comparison of the observed curves with those predicted by Volterra showed, however, that Volterra's equations must be modified, to bring them into agreement with observations. Now we may say 'better' equations were thus established.

In the field of nervous excitation considerable progress has been made by the University of Chicago group on the one hand, and by A. V. Hill and his school on the other. The following two graphs (Figures 3 and 4) illustrate the agreement between the predictions of the theory, based on a set of assumptions, and the experimental findings. The two graphs are taken from the work of A. V. Hill and his collaborators.

In genetics notable progress has been achieved through the cooperation of mathematical theory and experiment. We may mention the work of Sewall Wright, J. B. S. Haldane, Hilda Geiringer, and others.

BECAUSE OF the connection with the social sciences, we shall discuss somewhat more in detail the mathematical biology of the central nervous system and of behavior. The development of this field is associated with the names of H. D. Landahl, A. S. Householder, Anatol Rapoport, Alfonso Shimbel, and others. The development of the theory proceeded according to what we may call the 'standard' patterns discussed above. A set of fundamental hypotheses, of a rather simple nature, was made concerning the dynamics of interaction of any two neural elements, of which our central nervous system is made up. Then hypothetic structural arrangements of different degrees of complexity were investigated mathematically. The properties of some of those structures have shown amazing similarity to the properties of parts of our central nervous system. Thus gradually an elaborate theory was developed, which not only accounts for a number of observed quantitative relations but which in two cases led to predictions of new relations. The following graph gives an example. Here (Figure 5) we have a comparison of the theoretically derived relation between intensity of a stimulus and the reaction time to that stimulus, and the known experimental results. On the next graph (Figure 6) we have a comparison of

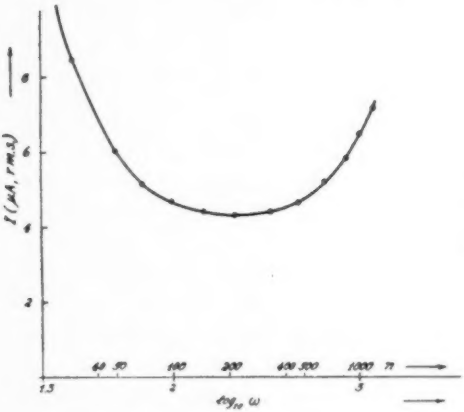

FIGURE 3. Frequency of alternating current (horizontal axis) plotted against amplitude just sufficient to cause excitation.

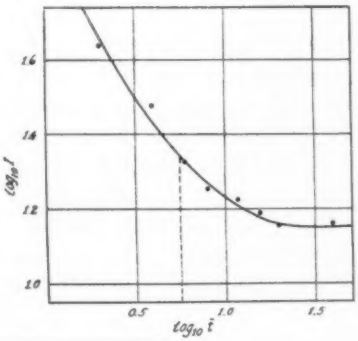

FIGURE 4. Pulse duration of a constant current (horizontal axis) plotted against current thresholds.

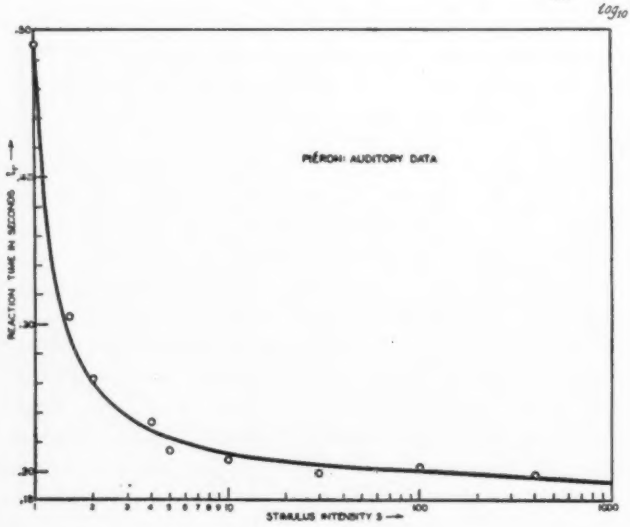

FIGURE 5

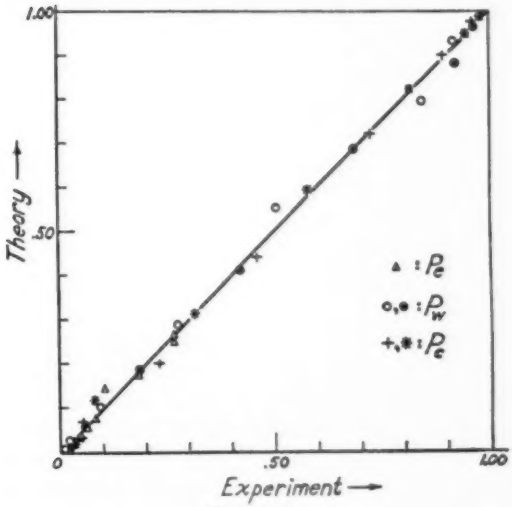

FIGURE 6

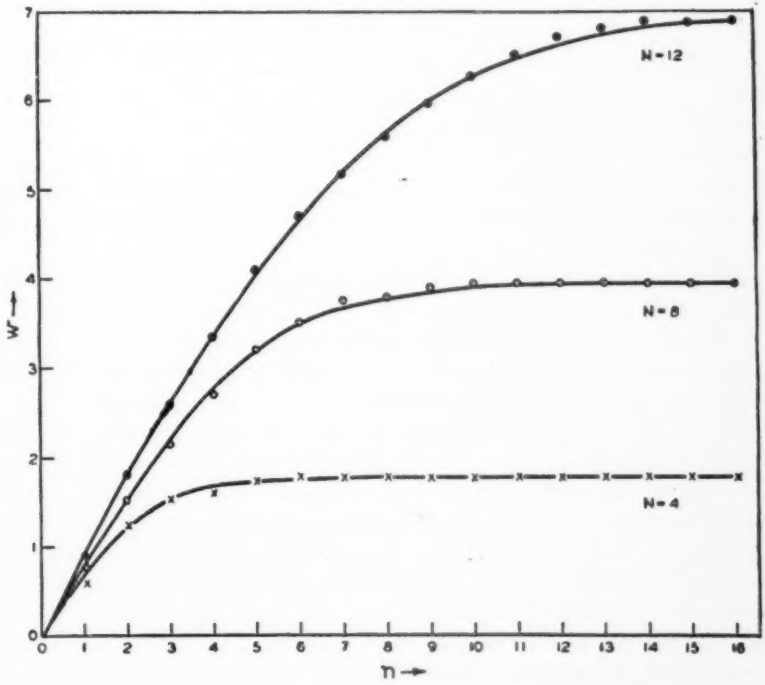

FIGURE 7. Errors (n) vs. trials (w) in a memory task. N indicates complexity.

theory and experiment in the study of the response of an individual to two stimuli which are sufficiently close to one another to cause the individual to occasionally confuse them. The percentage of confusion or errors can be theoretically calculated from the difference of intensities of the two stimuli.

The next graph (Figure 7) illustrates an application of the theory to learning. The mechanism of learning varies undoubtedly with varying types of tasks to be learned. A theory was developed by H. D. Landahl for a particular task, and a rather complex equation which determines the learning curve for different degrees of difficulty was derived. No data for this type of task were available. Therefore experiments were performed to verify the theory, with the results shown on the graph.

A mathematical theory of various emotional processes has also been developed. The results may be illustrated by the following graph (Figure 8) which shows a comparison of theoretically predicted and experimentally determined (by standard psychometric methods) relative aesthetic values of 77 different polygonal geometric figures. The theoretical prediction in this case was a real prediction, having been made more than a year before the experiments were started.

IT MAY BE objected, and it actually has been objected against the above-outlined mathematical theory of the central nervous system, that the postulates on which it is based are definitely known not to correspond to reality. But on that basis we may just as well object against the kinetic theory of gases because its fundamental assumption that molecules may be considered as rigid spheres is at least just as removed from reality as the postulates of the mathematical biology of the central nervous system. As we said above, no postulate ever corresponds to any 'ultimate reality.'

We thus have a fairly well developed mathematical theory of the neurobiological basis of human behavior. We can describe and predict how an individual will behave when confronted with certain more or less complex stimulus patterns. But no individual is ever completely isolated from others. In human society the actions of any one individual are part of the stimulus pattern perceived by the others. Thus, logically, the next step is to generalize the theory to interaction between two or several individuals. In this way we are naturally led into the field of social problems, perhaps into what may be called 'mathematical sociology.' If mathematical biology is possible, why not mathematical sociology? A ready objection is the allegedly greater complexity of social phenomena. But the same objection was raised in the past against the possibility of mathematical biology.

Attempts at creating a mathematical sociology are not new. We have in mind Haret's *Mécanique Sociale,* which dates back to 1910. Though this work superficially looks like a physicomathematical approach to sociology, in our

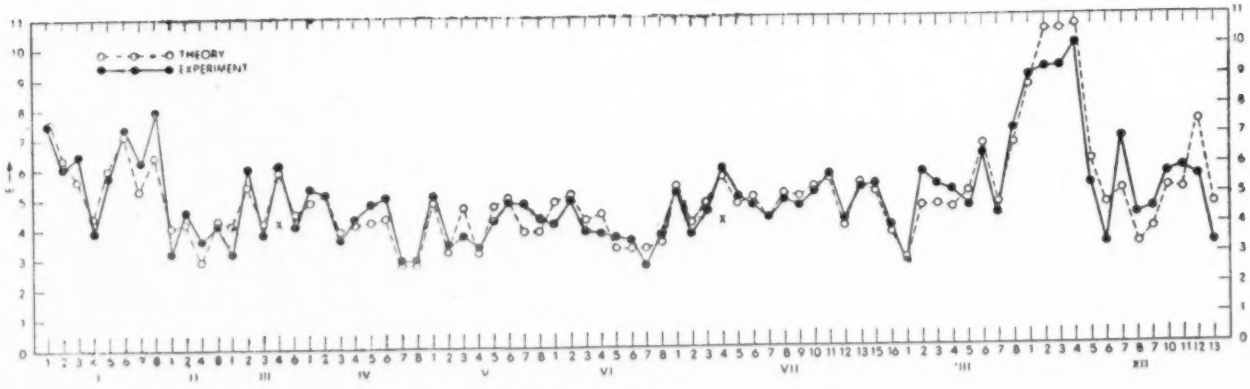

FIGURE 8

A mathematical theory of various emotional processes has been developed. The results are illustrated by the above graph which shows a comparison of theoretically predicted and experimentally determined (by standard psychometric methods) relative aesthetic values of 77 different polygonal geometric figures. The theoretical prediction in this case was a real prediction, having been made more than a year before the experiments were started.

opinion it exhibits a complete lack of understanding of the true spirit of the mathematical methods in science. What Haret does is to take the equations of classical mechanics and try to apply them to social phenomena, by introducing such artificial concepts as 'social mass,' 'social inertia,' etc. There is no reason whatsoever why social phenomena should be described by the same equations as mechanical phenomena. In biology we do not consider a cell as a huge molecule and do not apply to it Schrödinger's wave equation. Biological and social phenomena, while possibly having a common physical basis, are nevertheless *sui generis,* and are described by mathematical laws of their own.

The proper thing to do, in keeping with the spirit of the scientific method, is to derive from the theory of the nervous system the equations which govern the interaction of several individuals, and proceed to the development of a mathematical sociology. As this task is rather difficult, we may, *as a temporary expedient,* build a mathematical sociology on a set of special postulates which deal with directly observable social concepts or with abstractions thereof, but which do not blindly mimic the postulates of physics. An attempt in this last direction was made in our *Mathematical Theory of Human Relations.* A number of interesting consequences has thus been developed, and even a comparison with some quantitative sociological data has been made. Subsequently it was found to be possible to derive the special postulates from the mathematical theory of the central nervous system, and thus to follow the direct way. The results are summarized in our book *Mathematical Biology of Social Behavior.*

THE THEORY thus developed deals with quantities that are probabilistic or statistical in their nature. We already have seen that uniformities of such a nature are characteristic of the social sciences. It is impossible to predict with any exactitude the life history and behavior of a king, dictator, or other individual. But it is equally impossible to predict the exact course of an individual electron or atom. This latter circumstance does not worry the physicist. Similarly the sociologist need not be worried about the former. He can observe regularities between a large number of social quantities, such as, for example, divorce rates, crime incidence, church attendance, scientific output of a nation, etc. Mathematical sociology derives equations which relate to precisely this kind of quantities. Whenever enough data are available, they can be checked. If, as is at present preponderately the case, sufficient statistical observations are not at hand, the theory, even in its present very incomplete form, suggests definite quantitative sociological observation.

But, the sociologist may say, there are plenty of purely qualitative observations in sociology which are not amenable to quantitative analysis. For example, we may study the spread of ideas, beliefs, mores, etc., for which there is no quantitative measure. Well, isn't there really? We can measure, in principle at least, the relative number of individuals who accept given ideas or beliefs

at any given time. The increase or decrease of this relative number gives us the increase or decrease of the spread or 'popularity' of the idea. And, what is most important, the mathematical biology of the central nervous system shows that the above-mentioned relative numbers of individuals are closely related to the *average* values of certain neurobiophysical quantities characteristic of the individuals in the society, quantities which determine the psychophysiological 'drives' or 'preferences' of individuals toward one or another idea or belief.

Such concepts as 'social rank,' which can in principle be measured by appropriate psychometric methods, are given a neurobiophysical interpretation.

Of course, the value of qualitative observations cannot be denied. But the same may be said about their limitations as has been said about the limitations of qualitative relations in physics and biology.

Mathematical sociology seems to be well on its way as the youngest among the exact sciences. Like the older two systems, mathematical physics and mathematical biology, it will, if properly developed, lead to further improvement and extensions of the already existing quantitative methods of observation in sociology.

NOW FOR a few concluding words. As we have seen, the scientist is engaged in constructing a conceptual framework which is isomorphic with the observable world, and thus enables him to predict observable phenomena. To construct this framework, the scientist takes hints from observed phenomena. We may now, perhaps, indulge in a little wild speculation and ask whether it is possible that our own thought patterns are isomorphic with the observable world. Is there perhaps a set of equivalent conceptual frameworks which is the only one completely compatible with our laws of thought? If so, it might some time be possible to derive all observable phenomena from purely a priori postulates. Something akin to this idea seems to underlie the work of the late Sir Arthur Eddington, summarized in his posthumous book *Fundamental Theory*. The book has caused a great deal of controversy. Personally we prefer to preserve a noncommittal attitude. Should, however, the possibility envisaged by Eddington materialize, then the words of the great German poet, which we quoted before, may really turn out to be prophetic.

Believers in an absolute easily become monsters. It may be the concept of an absolute economic system, an absolute racism, or an absolute god. All require the shedding of rivers of human blood. OTTO PAULS.

SPEECH IMPULSE AND LANGUAGE FRUSTRATION

RICHARD D. LOEWENBERG *

FROM THE CRADLE to the grave we experience two antagonistic forces in steady conflict: one, a drive to reach out to others through sounds — to make connection across the deep chasm which separates human beings; the other, the awareness of how inadequate our tools are and how little, if anything, is transmitted to our fellow man. I call the first speech impulse, referring to the experience of the urge to speak, the conscious end result of what are ultimately physiological processes. I call the second language frustration or language distress, meaning all those inner experiences and responses which the speaker (and listener) suffers when he is conscious that he is unable to express himself so as to be understood by his fellow man. A discussion of these conflicting forces, I believe, can contribute some observations which belong to what Korzybski has called 'the study of the neglected forms of human behavior.' These two experiences of speech impulse and language frustration are strongly relived over and over. They are not deliberately chosen; nevertheless they are vividly felt at the most various levels of our emotions. Speech impulse and language frustration are inseparable, like love and hate, elation and depression.

This dilemma of spoken communication is experienced daily by everybody as the gulf between speaker and listener. It is especially marked between patient and physician, native and newcomer, parents and children, the artist and his public. I do not need to discuss in detail here the *Sprachnot* of young children or the stutterer's anxiety, so well known and much discussed. From my own experiences as a physician, working for a lifetime on three continents, among many different nations and tongues, I have observed a common denominator among many and varied speech experiences. Language distress appears as one of the fundamental experiences of human frustration.

Accompanying these experiences of isolation and withdrawal is a counter

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tendency to break through and overcome this terrifying frustration (anyone who listens to conversations of psychotics can observe these two forces). Manifestations of this reaction are of special interest to the general semanticist because they show patterns of language in the making, as in an experiment, pointing towards a possible future technology of language engineering. It is not my aim to present new material as such here; I hope to give a new focus by bringing together scattered facts and correlating the lessons of speech impulses and language frustration and pointing out the significance of *bridge languages* as a product of these forces.

Language Distress in Aphasia

THE MAGNIFYING distortion of pathology is especially instructive in demonstrating the interwoven play of speech impulse and language distress. The example of 'physiologically normal' aphasia—as in forgetting names, when every other detail is well remembered—can help us understand the deep distress felt in organic disorders. Compulsive indulgence in obscene words in children and the cursing compulsion of some metencephalitics show the close overlapping of functional and organic mechanisms. Not being concerned at present with the problem of the localization of cerebral lesions, I shall merely refer the reader to the excellent presentations of the problem by two distinguished authors whose approaches are diametrically opposed: J. M. Nielsen (11) and Kurt Goldstein (3). Both show little interest, however, in the question of language distress, which, as an internal experience and internal response, presents an effect similar to that found in organic disorders, even though its causes may be widely different. Kussmaul (8), on the other hand, has formulated sharply the neurosemantic process, so important for the learning of speech:

> Only practice establishes the connection between ganglion cell and ganglion cell, ganglionic center and ganglionic center . . . thus a central speech organ is gradually trained through language itself or, if you will, is created.

More than a decade before Freud's first publication Kussmaul clearly defined the unconscious mental forces in the development of speaking and came to the definition: 'Speaking means understanding—ourselves as well as others.'

It is not safe to assume that after the destruction of a supposed language formulation center a patient is mute because he has nothing to say. Unlike Thompson (17) we cannot find that senile aphasia patients peacefully accept their defect; unmistakably they manifest signs of distress as do other organically handicapped (those with tumors, Parkinsonism, etc.). They are often painfully conscious of their inabilities and say so after their recovery. A 65-year-old cerebral thrombosis patient complained to me the other day: 'I felt as if I were disconnected. When I tried to talk, the doctors crowded me in by their rapid

questions.' Stendhal (1783-1842), the French author, has given a striking description of an episodic aphasia in a letter of April 5, 1841, to Deifiori, from which I quote, after Birnbaum (1):

> I have touched nothingness. . . . Suddenly I forget all French words. I can no longer say: 'Bring me a glass of water.' I observe myself curiously. Aside from the use of words, I enjoy all the natural characteristics of the animal. This lasts eight to ten minutes. . . . The thoughts are completely in order but without words.

Balder Olden, a brilliant German writer, called his stroke the dividing line 'before and after my death.'

The comparison of aphasia with schizophrenic language disturbances has been a challenge for a long time, although these types of neurological and emotional disintegration should not be too easily put in parallel. Despert (2) interpreted the well known neologisms of schizophrenics as a manifestation of language distress, as attempts to express their strange new experiences arising from censored 'instinctual impulses.' Sullivan (Kasanin, 5), observes ingeniously that 'the safer we are with our fellows the simpler and also the more autistic becomes our operation with language symbols.' (Old friends and long-married couples understand each other silently!) He feels that the schizophrenic tries in vain to attain a feeling of security in 'the presence of strangers.'

IT IS NOT necessary to dwell here on the philosophic aspect of the inadequacies of language. These are clearly shown everyday, for example, when one wants to describe a face or has to listen to music appreciation talks. A few great poets have seen with uncanny clearness the limitations of their material, not only as Goethe did in regard to German, as one language, but as Schiller did in regard to all human languages. (Their point of view is markedly different from the usual praise of the mother tongue in most English, Romance, and Slavic poets, and, by the way, it dates back half a century before A. B. Johnson's famous treatise.) In a letter to Koerner, Schiller (16) wrote:

> The medium of the poet is words — that is, abstract signs for kinds and generic notions *(Arten und Gattungen)*, never for individuals. . . . Their relations are determined by rules, systematized by grammar.
>
> Words, as well as their laws, declensions, and connections, are entirely general things that do not serve one individual but an infinite number of individuals as signs. . . . Language puts everything before the intellect, and the poet must present everything to the imagination; poetry wants vision, language gives only concepts.
>
> Language deprives, therefore, the object — whose presentation is entrusted to it — of its sensual vitality and individuality and impresses on it a quality which is foreign to it.

We might expect some illumination on the problem of linguistic frustration from the clinical evidence turned up in the abnormal sociological conditions so

common in these days of shifted populations. In Hungary and Russia the same word *(neme, nemet, nemic)* is used to denote a mute, who cannot talk, and a foreigner who cannot speak the language of the country. (Kastein, 6)

There is yet another field in psychopathology that can be only mentioned here: the language isolation of new immigrants, who, like the deaf, may develop paranoid reactions. I have observed this reaction in this country as have others in the Argentine. It may even be compared to the paranoid panic after enforced blindness, when for example occasionally after a cataract operation the healthy eye is also bandaged. The sense of loneliness, reinforced and precipitated by the exclusion of the most vital social sense organs, mobilizes mechanisms of self-reference.

Makeshift Vernaculars

AMONG the most fascinating experiences for every world traveler are the contact vernaculars, the bridge languages, or makeshift minimum languages, as Jespersen called them. I wonder whether present-day bridge languages have met with sufficient systematic scientific interest. Pidgin English — the Anglo-Chinese hybrid used throughout eastern Asia — is known mostly as a kind of joke. When I asked in China whether it was studied seriously, I was laughed at, although many millions speak it. None of the widely read language books (Gray, Bodmer, Morris, Johnson, Ogden,) spends much more than a paragraph on this subject, if it is treated at all. Some authors, however, like J. E. Reinecke (13, 14a) and Robert Hall (14b) have done valuable pioneer work.

During both world wars the Anglo-American occupation of Italy and the German occupation of Poland created soldier languages (4). In the former case the British and the American troops in Italy created a simplified Italian — not a basic English! Unfortunately I have found no systematic studies, nor anything about the camp languages in Germany, where Czech, Polish, and Norwegian children formed a boys' language of their own.

Primitive people are convinced that their mother-tongue is completely a part of every human being, like breathing or walking. Even well educated people can hardly get rid of this feeling. As Korzybski says, 'Educated Anglo-Saxons are . . . handicapped with the knowledge of their language by being born into it.' (7)

The practice in radio plays of having foreigners talk like Germans, Russians, or Latin Americans in a thick accent seems on the surface an ingenious device. Its effect on the naive listener, however, might be to reinforce his childish belief that, after all, these foreigners are morons who only talk a gibberish — whose approximately correct English reveals them as only approximately human.

How language influences our entire perception and experience will be clear to every newcomer in a foreign country: when he has a chance to speak his

mother tongue he may notice that the whole scene feels closer to him and clearer than when he is painstakingly stuttering the new language.

Synthetic Languages

THE LEARNING of a second language is perhaps the most instructive illustration of the feeling of language distress under normal everyday-life conditions. Fortunately some people don't forget the years of ordeal they suffered before they were able to master a new language. It takes a long time before the stage of enjoyment is reached at which a new opportunity of expression is welcomed. It is not only the poorly gifted learners of a language who are subjected to this experience of frustration. On the contrary, the most polyglot individuals have become convinced of the feasibility of a synthetic language, which they themselves needed least. Schleyer, the priest who started Volapük supposedly mastered fifty languages. Max Müller, Sir William Ramsay, and Jespersen are only a few others in this illustrious series.

After reading in an English and a French book, without noticing his switching from one to the other, the 18th Century German physicist, Lichtenberg (9), asked himself in his diary (October, 1797),

> Is it good and advantageous for my mind to get so accustomed to it? I cannot possibly believe it. I do not mean the loss of time — which is obviously very great; but I believe that it is psychologically harmful to have in my head so many signs for the same object. Possibly we may imagine new qualities to exist when an old sign is supplanted by new ones. While I was passing from the English to the French work, an entirely new register had to be pulled and still I did not notice it. I would like to read a study on this.

E. Thorndyke (Connor, 18) of Columbia University demonstrated in extensive tests that the synthetic languages are five to fifteen times easier to acquire than the natural languages. However, there is one point to be stressed. The acquisition of any streamlined language still requires time, just as learning how to drive, to take pictures, to typewrite, or any other skill does. Our belief in shortcuts has made us blind to the need for time in any mental activity. Morris (10) recommended synthetic languages as especially suitable to study the process of language learning.

How do the bridge languages, such as Pidgin English and the 'basic Italian' mentioned earlier, bridge the differences between languages of different structure? Why are translations, although much maligned, often much better than any theory would lead us to expect? How has nationalism influenced the revival of dead languages? Which synthetic language creations have proved their living value? Is their success due to their structure or to outside forces? Is every new language perhaps artificial in comparison to our mother tongue? Was Nietzsche right when he said:

> In some far-off future there will be a new language, used at first as a language of commerce, then as a language of intellectual intercourse, then for all, as surely as sometime or other there will be aviation. Why else should philology have studied the laws of language for a whole century and have estimated the necessary, the valuable, and the successful portion of each separate language?' (12)

CONTRARY to public opinion, an international language confines itself to being an auxiliary language without ever being intended to suppress or to supplant national languages, or to become the vehicle for esthetic, literary expression. The shades and overtones, the allusions and nuances of a natural language are not a part of the task of an auxiliary language. And let us be honest; the high literary styles of Dante, Shakespeare, Goethe are very, very rare indeed. There are few scientific and journalistic communications nowadays that pretend to such a high literary level. But the power of language is so irresistible that even artificial straight-jackets can never suppress it entirely; at least one of the synthetic languages, Esperanto, has given remarkable proof of high literary possibilities in its abundant literature of translations. Speech impulse and language frustration are the creators and guarantee of the possibility of a streamlined world interlanguage.

Essentially, we are restating what history has already confirmed. Zamenhof, a physician, born in a city of four different languages, created in his Esperanto the only really living international language. He was, not incidentally, a Jew. So far, Esperanto has not been examined from a general semantic point of view. Its ingenious morphology and its development over sixty years would seem to justify such an evaluation. This scrutiny should be based not on old-fashioned grammatical and scholastic criteria, but upon the facts of language life, as shown in psychopathology and comparative linguistics.

The Creative Power of Speech Impulse

EVEN the word 'international' is quite young. It was introduced by Jeremy Bentham in 1780. Therefore, we should not despair. In thinking about an international language we should remember that no new tool, no new method that procures new data and opens new experiences, should be looked upon with contempt. All language activities are an incessant translation, an infinite chain of reactions. In spite of all our failings, the urge to understand, to join, and to share, survives and reasserts itself.

We cannot help carrying on certain practical linguistic transactions, irrespective of theoretical objections. Deep within us we feel that it is possible to cure, to educate, to govern, and even to understand people — if not perfectly, certainly infinitely better than has been done so far.

As a physician I started from my experiences with my patients, the sufferers who suffer doubly when they are deprived of their tools of expression. If this

is true of the individual, why should it not be true of nations too, that stare at each other helplessly like mutes?

Whatever the sources and causes of language distress may be, the drive to speak always asserts itself against even the strongest obstacles. Irrespective of all scientific arguments and prophecies — and sometimes even in spite of the tailoring and doctoring of scholars — this creative power lives in the oldest and newest languages, the revived and the constructed ones. Whether the infant cries out for food, whether the aphasic gropes for sounds to give meaning, or whether natives and traders develop their makeshift minimum languages, the linguistic primate breaks through his isolation again and again into the world of communicative symbols, without which we cannot exist.

REFERENCES

1. K. Birnbaum, *Psychopathologische Dokumente* (Berlin: J. Springer, 1929) 280.
2. I. Louise Despert, *Monatsch. f. Psychiatrie u. Neurologie,* 104.193 (1941).
3. Kurt Goldstein, *Language and Language Disturbances* (New York: Grune and Stratton, 1948).
4. *Heraldo de Esperanto,* April 1, 1948, and June 10, 1948.
5. I. Kasanin, *Language and Thought in Schizophrenia* (Berkeley: University of California Press, 1944).
6. S. Kastein, in *Twentieth Century Speech and Voice Correction* (New York: Philosophical Library, 1948) 72.
7. A. Korzybski, *Science and Sanity* (3rd ed. Lakeville, Conn.: International Non-Aristotelian Library Publishing Company, 1948) 22.
8. A. Kussmaul, *Die Stoerungen der Sprache* (1881).
9. George A. Lichtenberg, *Werke* (ed. R. Goldschmitt, Stuttgart: Walter Haedecke, 1924) 76.
10. Charles Morris, *Signs, Language and Behavior* (New York: Prentice-Hall, 1946).
11. I. M. Nielsen, *Agnosia, Aphaxia, Aphasia* (2nd ed. New York: Paul Hoeber, 1948).
12. Friederich Nietzsche, *Menschliches, Allzumenschliches* (Leipzig: Alfred Koerner, 1930) 218.
13. John E. Reinecke, 'Pidgin English in Hawaii,' *American Journal of Sociology,* 43. 778 (1938).

14a. John E. Reinecke, 'Trade Jargons,' *Social Forces,* 17.107 (1938).

14b. Robert A. Hall, Jr., 'The Linguistic Structure of Taki-Taki,' *Language,* 24.92 (1948).

15. George Santayana, *Persons and Places* (New York: Scribners, 1944) 149.
16. F. Schiller, in G. Landauer, *Skepsis und Mystik* (Berlin: Egon Fleischel, 1903), 147.
17. George N. Thompson, 'Fear Reaction Induced by Aphasia,' *Bulletin of Los Angeles Neurological Society,* 13.232 (1948).
18. E. Thorndyke, in G. A. Conner, *Esperanto, the World Interlanguage* (New York: Beechhurst Press, 1948).

To believe that what eventually happened must have been preceded by an intention, idea, or scheme, before it actually happened, is the prime fallacy of metaphysical thinking. JOHN DEWEY.

OPERATIONAL ANTHROPOLOGY AS AN EMERGENT DISCIPLINE[1]

LAURA THOMPSON

OUT OF THE RECENT increased interest in the human sciences are emerging new developments in theory and method. Among these is what for want of a better term will be called 'operational anthropology.' In this paper, I wish to suggest the potential role and significance of operational anthropology in the contemporary world.

Operational anthropology may be thought of as an emergent, integrative discipline which has not yet been theoretically systematized, but which has demonstrated its usefulness to a marked degree. This has come about because operational anthropology has developed mainly through the experimental use of certain concepts and methods—chiefly from anthropology, sociology, ecology, and psychiatry—clinically applied to large scale human problems, especially those of community and regional administration, wherein the need for solutions has been urgent. In the dynamic field situation, with the welfare and even the lives of large numbers of people at stake, certain orientations and methods of contemporary social science have proved operationally adequate; others have been revealed clearly to have limitations and inadequacies.

To grasp what is meant by operational anthropology let us recall certain relevant facts about the history of science. It is generally agreed that progress in the social sciences has been retarded by a dearth of hypotheses which might be tested in field or laboratory. The absence of testing procedures has reinforced a tendency of social science to remain divorced from the main stream of contemporary social issues. Thus traditional approaches have assumed the characteristics of dogma generally accepted and perpetuated by routine.

This has led in anthropology to the amassing of quantities of data, and to the elaboration of methods for the description, comparison and classification of observed phenomena as ends in themselves. This type of activity is generally regarded as 'objective' and within the tradition of 'pure research.' As such it

[1] The substance of this paper was prepared for inclusion in *Beiträge zur Völker- und Gesellungsforschung* (Berlin: Gebrüder Mann, 1950), celebrating the eightieth birthday of Professor Richard C. Thurnwald. The writer takes this occasion to thank Professor John Collier for valuable suggestions. The term 'operational' is here used with the limited and definite meaning given it in the literature on 'operational research' in England; i.e. research, nearly always interdisciplinary, directed toward practical problems of whatever scope, which omits nothing, including user-cooperation, needful for the full, practical solution of the given problem.

has assumed the character of a norm by which to measure the value of anthropological research, the merit of the researcher as a scientist, and consequently his academic status and advancement. Research projects oriented toward illuminating practical human problems automatically fall, according to this view, into the special category of 'applied research,' and it is commonly assumed that, due to its nature, 'applied research' cannot be as 'objective' or as 'scientific' as 'pure research.' [2]

The 'purist' position may best be understood within the positivist frame of reference which has long dominated social science research and is still the working hypothesis of most anthropologists. Positivist doctrine holds, as Whitehead says, that we can only know that which we can observe directly, and that natural law is merely an observed persistence of pattern in the observed succession of natural things. (32, p. 147)

The Weakness of the Positivist Outlook

TWO POINTS about the positivist school of thought have a direct bearing on the 'purist' position. The first is that this school offers no valid base whereby we may claim to have knowledge of the future. We can describe what *has happened,* but there all possibility of knowledge ends. Such a description, therefore, can be merely a *general* description and not an *explanatory* description which may serve as a basis for the speculative extension of laws beyond actual, particular instances of observation. According to Whitehead:

> The Positivist has no foothold on which he can rely for speculation beyond the region of direct observation. . . [This school of thought] has never been acted on. It can never be acted on, for it gives no foothold for any forecast of the future around which purpose can weave itself. (32, p. 159)

Insofar as application of social science toward the solution of practical problems usually involves some sort of assumption regarding our knowledge of the future, the positivist 'purist' is theoretically justified in his rejection of 'applied research.' Within the positivist framework of natural law as pure description, and of the limitation of knowledge to observed successions, 'applied research' involving any form of prediction would logically be classified as 'unscientific.'

The second point to be noted is that while positivism seeks to evade metaphysical speculation, many of its adherents actually do base their work on certain

[2] As John Useem has pointed out, 'Critics have said that studies by applied anthropologists are "practical" research rather than "fundamental," but it is significant to note that in the frequent references to fundamental in contradiction to practical, no definition is offered as to what the terms include nor proof cited to support the dichotomy . . . the term "fundamental research" may have a high prestige value as a scientific symbol but its contents require far more critical analysis than we have thus far subjected it to.' (31, p. 10)

metaphysical conceptions implicitly and hence uncritically entertained. A common assumption of this type is that sequences observed in the past will recur in the future, an assumption expressed quantitatively as permanence in statistical form. The outcome is a rigid kind of statistical causality, or its equivalent through repetition of observed patterning—a notion which underlies much of today's social science, especially applied social research.

This orientation is frequently implicit, for example, in the concept of applied anthropology as 'social engineering' and of the role of the applied anthropologist as a 'social engineer' or 'manager' of human relations. We may expect that if our knowledge is limited to observed sequences implicitly assumed to recur in the future, applications of such knowledge will inevitably tend toward the manipulation of such directly observed patterns or statistical forms for specific purposes. Indeed, as Erich Fromm has pointed out, 'For manipulative purposes, all that is necessary to know is the surface features of things, the superficial.' (10, p. 75) According to Whitehead's definition, such surface knowledge, and such knowledge only is available to the positivist.

Right here, it appears, lies the prime conceptual weakness and also the practical danger inherent in positivistic social engineering and related types of applied social research. Within the theoretically limited frame described above, applied social research is likely to develop in the direction of more precise *manipulative* skills toward short-range ends—*ends not derived from the internal structure of the situations which an attempt is being made to change, but rather ends superimposed from without by purposes specialized or extraneous,* such as the profit motive, the convenience and inertia of military and civilian bureaucracy, and cultural lags which an authoritarian state seeks to interject. Thus, in this type of applied research, scientific problem and administrative or managerial purpose are essentially unrelated.

Social engineering of the type defined above is employed in many types of applied social research. It has been found to be effective, for example, in industrial research where the aim is to improve human relations within a particular economic organization of limited scope for the purpose of reducing interpersonal friction and increasing production and profits. It has also been effective to some extent in work with small, organized or informal groups: for instance, in endeavors to improve inter-group and 'race' relations within associations and neighborhoods in urban communities and within small towns and temporary camps such as the War Relocation Authority centers; and it has been used in government administration for the purpose of improving morale and interpersonal relations within an existing framework of organization and policy.[3]

[3] Numerous illustrations of this type of applied social research are found, for example, in the files of *Human Organization* (formerly *Applied Anthropology*), *Journal of Social Issues,* and of the War Relocation Authority.

THE POINT to be emphasized here is that, in applied social research of the positivistic social engineering type, *the values or ends to which manipulation is oriented do not emerge from the research itself but are extrinsic to it. Hence knowledge tends to become an instrument to be used for the better manipulation of persons and things.* Moreover, such research tends in the long run to attract and foster scientists of the so-called 'realist' type described by Fromm:

> . . . the perfect 'realist' sees all there is to be seen of the surface features of phenomena but . . . is quite incapable of penetrating below the surface to the essential, and of visualizing what is not yet apparent. . . . Reality to him is only the sum total of what has already materialized. This person is not lacking in imagination, but his is a calculating imagination, combining factors all of which are known and in existence, and inferring their future operation. . . . The 'realist' . . . sees the manifest world and he can act by manipulating things and people as they appear in this picture. . . . His view of reality is so distorted because of its lack of depth and perspective that he is apt to err when more than manipulation of immediately given data and short-range aims are involved. (10, pp. 89-90)

Of course I do not mean to imply that all social engineers are extreme 'realists' in Fromm's sense of the term. This would be as obviously absurd as to hold that all applied anthropologists are positivistic social engineers. But Fromm's portrait of the perfect 'realist' serves to highlight a trend which may be expected to become marked wherever positivistic social engineers develop skill, precision, and power.

However, such social engineering has definite and definable limitations. These limitations emerge clearly when attempts are made to solve long-range problems involving the whole ecological web of life, including man.

Most of the practical problems oriented toward the improvement of the welfare of a total community over a long period of time are of this type; those, for example, of the United States Indian Service (6), the Tennessee Valley Authority (5, 16), the Soil Conservation Service (22), and the Pacific Trust Territory (25, 27). The urgent need to translate such long-range, practical problems into scientific problems and to solve them operationally by means of the methods of science is forcing the anthropologist to extend his frame of reference from a limited one involving a small group which may be directly observed in the context of existing institutions within an existing society, to a frame encompassing all the relevant relationships of a complex, emergent life-web event in total geographic setting and historical perspective (12, 21, 24, 26, 30).

The clinical test, involving the life and welfare of local communities deeply rooted in nature and tradition, compels him to relate practical long-range aims to ecological, psychological, and cultural processes. In other words, the necessity to solve the problem clinically in the broad framework of living reality requires him to move beyond the positivist social engineer's type of approach.

It necessitates the discovery and the *explanatory* description of all relevant factors, *whether or not they may be directly observed,* in the emergent life situation in environmental context; it calls for reinforcement or reconstruction of their evolving internal relationships in line with the inherent 'directiveness of organic activities.'

In other words, to solve the problem in action and not merely on paper, the anthropologist has to ignore the boundaries between traditional academic disciplines, to experiment with promising techniques wherever they can be found, and to develop an adequate, multidiscipline methodology (29). He has to enlist the cooperative participation not only of scientists from more than one discipline but also that of the potential 'users' of the research; he has to train both the participant scientists and the 'users' to work together as an 'action research' team (8; 15, ch. 13; 28).

Finally, as a last decisive step, solution of the problem leads to the discovery of intrinsic norms and goals in the life-web process itself and calls for the identification of man-made purposes with those inherent norms and goals. Witness, for example, the substitution, in resources conservation work, of predominantly ecological and cultural approaches for predominantly academic or engineering approaches. Paul B. Sears says:

> Taking our evolutionary material and climate as relatively fixed, we may say that within each province [life, earth, water, air] all of the diversity of living communities represents a series of changes or stages toward an ideal limit. This limit is a stabilized climax, on mature topography, covered with a mature soil profile, in equilibrium with the given climate, fauna and flora. All other communities may be judged and designated in terms of their degree of approach to that limit. . . . Once this viewpoint is understood . . . a very practical concept in the utilization of the landscape by man becomes available; for in nature the theoretical climax expresses . . . the most effective possible relationship of life, climate, topography, and soil in producing the means of continuing life. Whatever promotes an approach to this ideal increases the capacity of the earth to support life steadily and continuously; whatever delays or reverses the process lessens that capacity. (20, p. 100)

This change in approach from social engineering to the search for nature's innate goals in order to harmonize man's purposes and activities with them is well illustrated in the work of the United States Soil Conservation Service, the Tennessee Valley Authority, and perhaps most richly in the development of the Histadruth in Palestine.

Necessity of Insight into Structure

COMPARED to positivistic social engineering, operational anthropology demands a cooperative, multidiscipline procedure, involving fundamental research throughout. Within this frame the positivist's dichotomy of 'pure

research' versus 'applied research' is invalid. Operational anthropology is simultaneously both 'pure' and 'applied.'

Operational anthropology, however, has this in common with positivism. Both hold that 'all scientific progress depends on first framing a formula giving a general description of observed fact.' (32, p. 164) It departs from and transcends positivism in that it proceeds from a formula describing observed fact to a deductive theory which attempts to *explain* observed fact. In contrast to positivism's rejection of metaphysics, this deductive theory has an implicit metaphysical base. It is rooted in what may be called a philosophy of holism. (23)

Holism has grown out of a preoccupation with developing organic life in its total setting. According to this school of thought, law is conceived as immanent in nature rather than (1) as mere observed sequence which may be the result of aimless fortuity, or (2) as power superimposed from without by a transcendent or supernatural force. (32, p. 142) Furthermore, immanent law as conceived within the holistic orientation is primarily a formative process rather than, for example, repetitive or rigidly reproductive process. (33) As manifest in a tendency in nature toward the formation of wholes, in the directiveness of organic activities, etc., it is basically *one-way* and *irreversible.*

Within this frame the main task of the sciences of man becomes that of understanding (that is, describing in an explanatory manner) the human race as part of the inherently lawful, dynamic natural order or emergent pattern of events. In other words, from this viewpoint, the aim of the human scientist is to understand man in relation to the world of nature, including other men, animals, and plants in process of development, formation and transformation. Significant problems in the sciences of man, as in other sciences, become problems of emergent structures; that is, problems of *changing relationships,* of *interacting forms, of emergent patterns.* (14, p. 60ff) Solution of problems of dynamic structure depends on insight into exact patterning. In the non-human sciences the solution of such problems frequently depends on exact numerical measurement. In the human sciences, solution of such problems frequently depends not on exactness of numerical measurement but rather on precision of relationship or on proportion of configuration irrespective of dimensions, or magnitude, or quantity. Hence their solution depends primarily on *structural insight.*

SUCH PROBLEMS, in their nature, cannot be solved solely by quantitative methods, and an attempt to use only such methods leads either to an artificial restriction of the field of investigation or to a mistaken identification of a *general* description of superficial appearances with an *explanatory* description of the phenomenon as a whole. However, the possibility that such basic structural relationships in the human sciences may be expressed in terms of non-numerical mathematics has been suggested, for example, by H. A. C. Dobbs who writes:

> It is obvious that the human sciences must remain non-metrical until such time as we have devised measuring scales and standards with the requisite geometrical structure. At present we are only prepared to call things like copper bars and bits of wood calibrated by Euclidean linear point series 'standard' measuring scales. . . .
>
> But that does not mean that the data or subject matter of human sciences are *incapable* of possessing the requisite structure for a mathematical treatment by non-metrical methods such as those employed in Analysis Situs, and indeed I believe that an important step toward such a treatment is to be found in Kurt Lewin's *Topological Methods in Psychology*. (8, p. 20)

Research along these lines is going forward and definitive findings may be expected in due course. The practical usefulness of operational anthropology, however, does not depend on any, even topological, mathematical formulation of the findings, although such formulation may be expected to accelerate its development.

In this context it need hardly be emphasized that the relationships determining the emergent patterns with which the operational anthropologist is primarily concerned are internal rather than external, covert rather than overt, inferred or projected on the basis of direct observation rather than directly observed. Thus, theoretically their investigation does not fall logically within the positivist's frame or area of preoccupation. But since practice in the sciences of man has far outrun theory, significant discoveries have been made concerning the relationships of man to nature and man to other men by social scientists concerned with practical problems involving the saving of human lives and the advancement of human welfare (including group physical and mental health), for example, in problems in social therapy (9, 13, 15), in colonial and community administration (7, 11, 25, 26, 29), and in resources conservation (6, 22, 27).

Although certain older attitudes have not as yet been discarded explicitly in favor of formulations more in accord with the new discoveries and the known facts, practical experience in laboratory, clinic, and field has pushed out the boundaries of the human sciences until they have outgrown their presuppositions.

Inorganic and Organic Equilibrium

A REVEALING illustration of the persistence of an inappropriate and confusing mode of thinking is found in the term 'equilibrium,' as still used by many social scientists. This term is often used by anthropologists, especially social engineers, to denote a state of balance in human relations or in social organizations. As is well known, the term 'equilibrium' may be used to express two entirely different types of balance: that which is characteristic of inorganic systems and that characteristic of organic systems. (19, p. 7) Equilibrium in inorganic systems refers to an end-state reached by a reversible process of 'closure'

and is characterized by a stable condition of least action. On the other hand, equilibrium in organic units refers to an end-state of irreversible, directive, developmental activities which is highly complex, unstable, and self-regulatory. Anthropologists, in referring to organic systems or to the behavior and symbolic systems of organisms, commonly use the term 'equilibrium' in such a way as to imply the stable state of balance of inorganic systems.[4] Academically, such an analogy may seem harmless. Clinically, however, it may have far-reaching and disastrous consequences.[5]

Fortunately, large-scale practical problems, involving complex organic types of organizations in their natural setting, are gradually forcing anthropologists to discard inappropriate analogies and to revise their basic concepts. Although this conceptual revision is in process, with a long way yet to go, a few examples will illustrate its general trend.

Specifically, the traditional positivistic definition of 'culture,' in terms of sum-totals or aggregates of observable culture 'traits' and observable behavior patterns, is beginning to give way to dynamic holistic formulations. For instance, within a holistic, developmental frame, a human culture may be conceived as a genetic, multidimensional, emergent pattern of related (psycho-biological, sociological, symbolic, etc.) human events which tends in the course of time to be integrated with the total environment by its human component. Seen thus, any one human culture becomes part of a web of comparable genetic patterns of human events in process of formation—some, like Western civilization, extending over huge central areas of the globe; others confined to small marginal localities; but all conceived as exceedingly complex, subtly interrelated, and moving forward as structures into the future out of the past through the operation of irreversible, inherent formative processes. (30) In this context a cultural group is conceived as the interacting organisms who comprise the local human component of the culture at any one point in its historical time span, and who express the culture, transmit it, and participate in its creation. Similarly, a cultural crisis is conceived as the manifestation of critical imbalance (of the organic type) in one or more essential dimensions of the culture pattern in environmental setting. (1, 2, 30)

In like manner a behavioristic definition of 'personality' in terms of 'social stimulus value,' a definition which has had quite a vogue among social scientists, including anthropologists—doubtless because it conforms to positivistic doctrine

[4] For example, according to Chapple and Coon, 'Equilibrium may be defined as a state in which, if a small force is impressed on a system, the force will produce modifications to a greater or lesser degree within the system, and when the force is removed, the system will tend to return to its previous state.' (4, p. 14)

[5] It is significant therefore that Chapple suggests that his 'theory of equilibrium in human relations' be used as the basis for the development of a practical scientific code of ethics (3, p. 79ff), and that it has actually been used as the basis for the Code of Ethics of the Society for Applied Anthropology. (17)

—is being replaced by integral, field concepts. For instance, for purposes of structural analysis a human personality may be conceived as the genetic relation between occasions of bio-social experience of a developing human organism interacting with other organisms and with natural environment. (18, ch. 39)

The emergence of a more adequate metaphysical and conceptual base may be expected to unveil new perspectives in anthropology. Many anthropologists now realize that anthropology is not likely to remain a disjointed 'study of man,' consisting of a number of separate 'fields of study'—e.g., social anthropology, physical anthropology, archaeology and culture history, ethnology, linguistics, and applied anthropology—'fields of study' related loosely through their common subject matter, *Homo sapiens,* and through their accidental location outside of, or marginal to, other traditional academic disciplines. On the contrary, anthropology is apparently developing toward maturity as a unified, operational science, integrating the ecological, bio-psychological, and social sciences.

Responsibilities of the Anthropologist

ANTHROPOLOGY'S integrative function as a life-science places rather heavy responsibilities on the operational anthropologist. Besides being thoroughly trained in the traditional anthropological 'fields of study,' he should be grounded in functional biology and general ecology as well as the *gestalt* psychological disciplines, especially psychiatry. (24, p. 14) We may expect that gradually these studies will be systematized and unified through their bearing on operational anthropology's primary focus of attention, namely, the formative *biocultural* process in total natural setting and historical perspective.

The development of operational anthropology to date suggests that the discipline may be systematized for purposes of concentration into five dimensions or study areas: (1) the ecological dimension, namely, the relation of the life-web, including man, to its total environment (here would fall anthropogeography, primitive and comparative economics, technology as related to environment, etc.); (2) the biological dimension, namely, the cultural group as biological organisms in environmental setting (here would fall physical anthropology, somatology, 'constitutional types,' etc.); (3) the sociological dimension (here would fall social anthropology, etc.); (4) the personality dimension (including especially communal personality structure, 'personality and culture,' 'national character structure,' etc.); (5) the symbolic dimension (including linguistics and semantics, mythology, folk-lore, art, ceremonial, comparative religion and ethics, basic ideologies, etc.). All these areas converge in the study of the emergence, integration, transmission, interaction, crisis, degeneration, and regeneration of cultures in space-time through the operation of natural and human formative and creative processes.

The student of operational anthropology will also need to master the art of cooperative 'action research.' In 'action research' he operates both as a field

scientist and as a 'democratic' or 'integrative' leader, in Kurt Lewin's sense of the term. (15, p. 75ff) As key integrator of a team of scientists from diverse disciplines (e.g., ecology, psychiatry, and anthropology), whose function is to serve and work with the potential users of the research, his role is not only to fulfill many of the traditional functions of a field anthropologist but also to guide and direct the training of the field workers, to coordinate and help the team of scientists and lay participants in the cooperative 'action research' project, and to take a lead in the terminal analysis, integration, and interpretation of the project findings.

Toward Structural Insight

SUCH MANY-SIDED functions, in action skills as well as in academic techniques, apparently call for a special type of personality pattern. Ideally, the operational anthropologist needs a refined, balanced, holistic type of perception pattern which enables him to grasp complex wholes with the relevant factors in interrelationship. In other words, he needs structural insight. Such a perception pattern contrasts strikingly with the peripheral type of approach of the so-called 'realist'—an approach which, as has been noted, is likely to be fostered by the role and functions of the positivistic social engineer.

The type of personality and of training needed by the operational anthropologist participating in cooperative action research is clarified by the following statement by John Collier:

> . . . let me emphasize that this kind of research makes demands on the research worker that are far more severe than those made by the specialized and isolated kind. It requires of him a more advanced and many-sided training, and in addition a type of mind and personality which can sustain, in suspension, complex wholes and which can entertain—yes, and be drawn and impelled by—human values and policy purposes while yet holding them disinterestedly far away. . . . I . . . suggest . . . that in the direction here implied social science is now verging on a new epoch of methodology, of aim and of use, with all that this implies regarding careers for young minds and preparation for such careers. (6, pp. 300-301)

The most significant aspect of operational anthropology is that as a corollary to its basic premise—that law as formative process is immanent in nature—*goals inherent in the living phenomena under investigation are emerging from its findings.* Just as in medical science there emerge from investigations of the phenomena themselves in total relevant context, inherent standards and goals whereby the physician may measure the degree of balance or imbalance of a living organism and its movement toward or away from health and life; and just as in ecology there emerge inherent standards and goals whereby the ecologist-conservationist may measure the degree of balance or imbalance of a natural ecological community and its movement toward or away from a 'climax' condi-

tion; so also in anthropology inherent standards and goals are emerging whereby the operational anthropologist may measure the degree of balance or imbalance of a human community in full biopsychological, cultural, and environmental context and its progress toward, or its regression from, an ideal ecological-cultural condition.

The implications of this approach are, one may suggest, revolutionary. For it reveals how, by relating human purposes and values, or what Korzybski (14) called 'evaluations,' to nature's innate norms and goals, man can resolve the ethical dilemma that arises from the man-made dichotomy between 'science' and 'values.' It opens the way toward the development of a truly *human* science which may be applied systematically for the unequivocal benefit of mankind. And it holds the hope that an ecologically and culturally pluralistic, balanced world community may in time be scientifically formulated and even, to a degree, scientifically implemented.

BIBLIOGRAPHY

1. David Bidney, 'The Concept of Culture and Some Cultural Fallacies,' *American Anthropologist,* 46.30-44 (1944).
2. ———, 'The Concept of Cultural Crisis,' *American Anthropologist,* 48.534-52 (1946).
3. E. D. Chapple, 'Anthropology and Ethics,' in *Perspectives on a Troubled Decade: Science, Philosophy and Religion, 1939-49.* New York: Harper, 1950.
4. E. D. Chapple and C. B. Coon, *Principles of Anthropology.* New York: Henry Holt, 1942.
5. Gordon R. Clapp, 'The Administrative Resources of a Region,' *New Horizons in Public Administration: A Symposium.* University, Ala.: University of Alabama Press, 1945.
6. John Collier, 'United States Indian Administration as a Laboratory of Ethnic Relations,' *Social Research,* 12.265-303 (1945).
7. ———, *The Indians of the Americas.* New York: W. W. Norton, 1947.
8. H. A. C. Dobbs, 'Operational Research and Action Research,' *Monographs of the Institute of Ethnic Affairs* (Washington 1947).
9. Lawrence K. Frank, 'Social Order and Psychiatry,' *American Journal of Orthopsychiatry,* 11.620-26 (1941).
10. Erich Fromm, *Man for Himself: An Inquiry into the Psychology of Ethics.* New York: Rinehart, 1947.
11. Manuel Gamio, 'Las Necesidades y Aspiraciones indígenas y los Medios de Satisfacerlas,' *América Indígena* 11.105-12 (1949).
12. International Technical Conference on the Protection of Nature, Resolution No. 1, UNESCO (Lake Success, N. Y. Aug. 1949).
13. Alice Joseph, M.D. and Veronica Murray, M.D., 'Chamorros and Carolinians of Saipan: Personality Studies of Saipanese Children and Adults. Cambridge: Harvard University Press (forthcoming); cf., *Newsletter of the Institute of Ethnic Affairs* 4.1-4 (1949).

14. Alfred Korzybski, *Science and Sanity: An Introduction to Non-Aristotelian Systems and General Semantics*. Lakeville, Conn.: International Non-Aristotelian Library Publishing Co., 3rd ed., 1948.
15. Kurt Lewin, *Resolving Social Conflicts*. New York: Harper, 1948.
16. David E. Lilienthal, *TVA: Democracy on the March*. New York: Harper, 1944.
17. Margaret Mead, E. D. Chapple, and G. G. Brown, 'Report of the Committee on Ethics,' *Human Organization* 8.20ff (1949).
18. Gardner Murphy, *Personality: A Biosocial Approach to Origins and Structure*. New York: Harper, 1947.
19. E. S. Russell, *The Directiveness of Organic Activities*. Cambridge, England: Cambridge Univ. Press, 1945.
20. Paul B. Sears, *Life and Environment: The Interrelations of Living Things*. New York: Teachers College, Columbia Univ., 1939.
21. John P. Shea, 'Human Relations: an Essential Factor in Resource Protection and Use,' International Conference on the Protection of Nature, UNESCO (Lake Success, Aug. 1949).
22. Ward Shepard, *Food or Famine*. New York: Macmillan, 1946.
23. J. C. Smuts, *Holism and Evolution*. New York: Macmillan, 1926.
24. Laura Thompson, 'Some Perspectives in Applied Anthropology,' *Applied Anthropology*, 3.12-16 (1944).
25. ———, 'A Model Trusteeship for Micronesia,' *Newsletter of the Institute of Ethnic Affairs*, 3.6-8 (1948).
26. ———, 'Relations of Men, Animals, and Plants in an Island Community (Fiji),' *American Anthropologist* 51.253-67 (1949).
27. ———, 'The Basic Conservation Problem,' *Scientific Monthly* 68.129-31 (1949).
28. ———, 'Action Research among American Indians,' *Scientific Monthly* 70.34-40 (1950).
29. ———, 'Personality and Government: Findings and Recommendations of the Indian Administration Research,' *América Indígena* Vol. 10, no. 1, *et seq.* (1950).
30. ———, *Culture in Crisis: A Study of the Hopi Indians*. New York: Harper, 1950.
31. John Useem, 'Applied Anthropology in Micronesia,' *Applied Anthropology* 6.1-14 (1947).
32. Alfred N. Whitehead, *Adventures of Ideas*. New York: Macmillan, 1933.
33. Lancelot Law Whyte, *The Next Development in Man*. New York: Henry Holt, 1948.

"To be a successful detective, Oscar, one has to have something more than a knack of observation and deduction. One needs to be able sometimes to come to the right answer without going through all the laborsome preliminaries. Call it intuition or extra-sensory perception if you will —'

'But two and two still make four!'

'Two and two what? If you mean numerals they can just as well make twenty-two or zero, depending on how they're arranged. And two apples and two notes on a bugle don't make four of anything.' STUART PALMER, *Four Lost Ladies* (*a mystery novel*).

✡ OBITER DICTA ✡

THE WORSHIP OF P:
An Open Letter to a Brother Psychologist

J. S. A. BOIS[1]

MY DEAR JACK: You told me this afternoon that late last night you discovered some deep meaning in my reference, earlier in the day, to Psychology with a capital P. I assumed at the time that there had been a meeting of our minds on this subject.

Now, I am not sure whether we met on the same spot. This epistle is intended to describe to you where I stand and what I see from where I stand.

I stand in the center of my own world, of course. In the middle of the only world that is real to me. You stand in the middle of yours. So do our brother psychologists with whom we shook hands this afternoon.

As psychologists, we feel we all have something in common. At least, that's the way we say it: *something in common.* And this something we call *psychology.* When we take ourselves in earnest, we call it *Psychology,* with a capital P.

Tonight I am trying to get hold of that Psychology with a capital P (let us call it P for short), and I find the darn thing very elusive. It reminds me of what Poincaré said of the physicists of old who wanted to get hold of *heat* and could not, even when they gave it a Greek name (*phlogiston*) to conjure up its presence by 'scientific' means. You remember the sequence, don't you? 'Nouns are substantives. Therefore nouns refer to substances. Substances stand by themselves. Substances are material *things,* (unless of course they are "spiritual" like angels). Therefore, *heat* is a thing, an element like gold, iron, sulphur, etc.'

But the pesky thing did not answer the urgent beckoning of the physicist who called it out of his magician's hat — or was it his magician's head? Try as he may, he could not perform the expected trick. He called *heat,* he called *phlogiston,* and nothing came out but the hollow sound of the scientific wand on the magician's hat — or was it on the magician's head?

[1] Consulting psychologist, Montreal, Canada; president (1948-9), Canadian Psychological Association; lecturer, Institute of General Semantics Summer Seminar, 1950.

I AM AFRAID we are trying a similar magic trick when we want to conjure P out of our heads. There is something there. I imagine there is. But this something, is it *something?* Is it a goddess, like Minerva out of Jupiter's brain, which we must revere in blind obeisance? No, Jack, I won't have it that way. How can we be so anthropormorphic? 'Allons, allons!' a Frenchman would say.

'Well, what is it, anyway?' you ask me.

Word your question differently, my dear Jack. Otherwise I can't answer you. It is as if you divided a ruler in ten units and directed me to state in decimals what its exact third is. You know I couldn't answer that. Well, your question: 'What is *P*?' is just as unanswerable. As long as you dissect 'reality' in terms of 'what,' 'is,' and 'it,' you make it impossible for me to talk sense.

P is just a conventional symbol on paper, or a sound wave when I utter it as I trace these lines. To my six-year old daughter, P was a symbol for a type of activity that has little in common with the scientific outpourings we got at our annual meeting. To my wife, when she speaks to our canary, P means something different. To our president, when he gave his formal address, P meant *Put* of *Put away*.

To me, in the present context, it means a process, a happening, a 'going-on,' an electro-colloidal event, or a whole series of such events that fan out in a chain-reaction pattern. These events never stay put, and you can't take a still picture of them. They move too fast for our *camera obscura* (our brain, in common parlance), and our camera itself is carried away in the whirl.

So whatever picture we have of it is not a true picture at all. It is a man-made, static idol, made of brass like our laboratory gadgets or of paper and ink like our dissertations, this one included.

This process goes on in you, in me, in everyone who observes others — to forget himself, to bolster his ego, to get things done or undone, or just to make an honest living. It is in everyone who talks to himself about it, lectures about it, writes about it, or plays various little games which he calls experiments.

A COUPLE of generations back, some of those writers and experimenters went through a ceremony of self-anointing and gave themselves the rôle of keepers of the P. It was the spontaneous emergence of a sacred caste: *the high-priests of the P.* The rest followed as of necessity, like the conclusion of a syllogism once the premises are given: 'Since there are high-priests of the P, there is a P, pure, holy, and perfect in all respects.'

Well, Jack, call me iconoclast if you will, but know once and for all that I don't believe in P. I won't worship at the altar of P. I don't want to be identified with the high-priests of P.

Once, I was corrected by the editor of our *Journal* for writing something

that suited my conscience, but did not jibe with the tenets of our 'religion.' I had said in my presidential address, in May 1949: 'Go into the world of business and industry, and you will find in it more psychology than you expect.' When my paper appeared in the *Journal*, I read to my astonishment: 'Go into the world of business and industry, and you will find in it more room for psychology than you expect.' I confess that I was too cowardly at the time to restate my conviction in no uncertain terms. But my conscience has been pricking me ever since, Jack. Today I want to become a *clear*, as they say in dianetics. There *is* psychology in business and industry, yes, indeed. Not the kind we advocate, perhaps. But there is some, by Jove, and its capacity for prediction is not negligible, either. Whether they *make* things happen as they predict is another matter. By the way, how much of the eventual results of our counselling, directive or nondirective, is due to our manipulating the subject, the environment, or both?

The point of all this outburst, my dear Jack, is that the process of *psychologizing* has been going on for centuries in millions of human brains and nervous systems. It continues today with accrued intensity, to the point that at times I see it as a St. Vitus' dance. I wonder if the scaring increase of maladjustments that we broadcast so complacently is not due in part to our ostentatious worship of P, the goddess who ensnares its worshippers into neurosis? You remember the Great Charcot and the *grand mal* he unconsciously and unwillingly suggested to his all too 'predictable' patients?

As far as I am concerned, I will not waste a minute in the worship of P. I shall pick up my tools, those still very primitive tools that my academic brothers are forging and that practice has shown me how to handle, and I shall try my best to reduce, if possible, the percentage of maladjustments in this teeming crowd of humans marching on to better things. If the percentage of maladjustments increases, no sacrificial offerings to the goddess P will assuage my feelings of guilt. I shall hide for shame, and take to the pick and shovel for a living.

Yours faithfully and faithlessly,
Sam

. . . in things abstract, men but differ in the sounds that come from their mouths, and not in the wordless thoughts lying at the bottom of their beings. Herman Melville, Mardi.

☆ BOOK REVIEWS ☆

Science as Social Therapy

SCIENCE AND THE GOALS OF MAN: A STUDY IN SEMANTIC ORIENTATION, by Anatol Rapoport. New York: Harper and Brothers, 1950. xxvi, 254 pp. $3.50.

GENERAL SEMANTICS has been described as a philosophy of life which accepts, as its metaphysics, modern science rather than any prescientific scheme, and adjoins thereto an ethic which stands in as intimate a relation to science as may be. So stated, there is the tacit implication that science, in itself, has nothing to tell us about ethics. It may be immoral to use nuclear physics for war, and moral to use it for medicine, but the morality adheres to the application, not to the science itself; neither this nor any other branch of science can tell us what is right, what is wrong. When a scientist is a good man, he is *also* a good man — not good because being a scientist requires it.

Such a view is common enough, though far from universal.[1] Rapoport rejects it. His purpose in this book is to demonstrate that, if we accept science as our metaphysics, we have no freedom in the choice of an ethic; that, rather, the scientific orientation *forces* our choice: 'This book is an attempt to show that the scientist, in order to be consistent, must subscribe to certain values (and discard others) not because he is a "good citizen" or a product of [a particular] culture, or a member of [a particular] church, but because he is a scientist' (p. xxii).

The last chapter is a partial summary of the argument, which runs about like this (pp. 224ff):

> A fundamental value in the scientific outlook is concern with the best available map of reality. The scientist will always seek a description of events which enables him to predict most by assuming least. He thus already prefers a particular form of behavior. If moralities are systems of preferences, here is at least one point at which science cannot be said to be *completely* without preferences. Science *prefers* good maps.

[1] The existence of such an organization as the Association of American (Atomic) Scientists implies, at least, that many scientists are disturbed about such a theory of the ethical implications of science. Max Otto more explicitly disagrees with the theory in *Science and the Moral Life* (New York: Mentor Books, 1949, reprinting various earlier articles and chapters), but in a rather different direction from Rapoport's.

There are obstacles in the search for truth (= good maps); the necessity of removing these obstacles defines 'a general direction for the establishment of an ethics.' That which helps the search for truth will be considered (relatively) 'good,' that which hinders will be considered (relatively) 'bad.' This argument applies to all science; a further argument applies specifically to certain branches. There is a relationship between 'sanity' in an individual and in a society: 'The measure of the sanity of an individual . . . is the correspondence between reality and the map the individual makes of reality.' We may define the sanity of a society 'in terms of the opportunities provided by its institutions to its members for the achievement of greater [individual] sanity. . . . The sanity of the individual is the concern of physicians.' The pathologist, of course, accomplishes nothing by labeling what he finds under a microscope 'evil'; this is an insufficiently specific response. But medical science *as a whole* must be concerned with the health of individuals, which is an ethical orientation. Similarly, the ethical orientation of social science *(as a whole)* must be based on a concern for social health (sanity). And now, 'Since [in all these cases] the values are not arbitrarily chosen but flow out of scientific practice itself, the scientist cannot discard them without becoming inconsistent, that is, without negating his own activity.'

RAPOPORT'S line of reasoning is at least persuasive, and perhaps conclusive. Certain reservations have occurred to your reviewer, and may occur also to other readers; they deserve mention here precisely because their relevance is — I think — only apparent.

(1) Whether or not there is an ethic which flows inevitably from scientific practice, should we not reserve the term 'science' for that aspect of the scientist's behavior in which he is not making value judgments, and call the remainder 'applied science' or 'art' or 'engineering'? Clearly we have a choice of terminology; but Rapoport's argument can be stated in either terminology and is as valid in one as in the other. Here, as so often, care has to be taken to keep terminological preferences from throwing up a smokescreen through which we cannot examine what really counts.

(2) As a matter of historical record, many important scientific contributions have been made by individuals who were anything but scientific in their life outside of working hours; there will be more such cases in the future. Granting that such an individual is not consistent — that he compartmentalizes himself — can we be at all sure that he would be a better scientist if he were indeed consistent throughout? Some individuals, at least in some cultural settings, seem to need a kind of anchor to windward which it is not possible for the scientific orientation to provide. Is it not, therefore, a fact that science has been

carried on in many cases *without* implying the choice of one set of values rather than another? Our answer must be affirmative, but it requires very little reflection to see that Rapoport's argument is not affected. Examine the life and cultural background of an individual of the kind mentioned. Often enough, his only choice has been between a compartmentalization, on the one hand, which allowed an island of rationality in an essentially irrational setting, and, on the other hand, no compartmentalization and little or no rationality. Rapoport's concern is with the enlarging of the range within which rationality is possible; the factors which have necessitated compartmentalization in the past will tend to diminish exactly to the extent that the implications of Rapoport's argument are generally realized. The method of science is a force remaking our culture in the direction of greater consistency and rationality.

(3) Granting that science generates the general contours of its ethic, is not much of the force of this lost because there are so many specific areas of choice where it gives us no help? A study of nuclear physics does not help us in deciding whether to be pro- or anti-communist. (The task of *managing to study* nuclear physics under varying cultural conditions does help us — but this is a different matter, impinging on social science.) Is there any branch of science which can tell me, when I need to have the answer, whether to vote for candidate X or candidate Y, whether to eat Cornies or Oaties for breakfast, whether or not to give up cigarette smoking, and so forth? Any choice has, potentially, an ethical angle, though often the answer may be 'it doesn't matter.' Now we must recognize that the development of science is highly uneven. Currently we know a good deal about physics, something about biology, little indeed about human behavior. At a given moment, an individual who must make a choice cannot have at his fingertips all the possible relevant results of science as of that moment; and if he could, there would often be blanks at just the crucial places. A society, in which individuals can pool their scientific experiences, can be similarly confronted. This would be true even were Science as a Way of Life much more generally the theme of our culture than it yet is. So spot decisions, best guesses, hunch-plays, will always have to be made, and some of them are always going to turn out to have been wrong. Far from being an argument against Rapoport's thesis, this demonstrates the need for ever more integration of human life in terms of science, so that wrong decisions can be continually reduced in number and relegated to areas of least importance.

RAPOPORT does not confine his attention narrowly to his announced theme, but — happily — dwells in considerable detail on various aspects of the scientific method and their relation to matters of everyday life. Your reviewer feels that on some points he goes astray, though not in a way to vitiate his main argument. The rest of this review is devoted to five such points; they are points

which are, or ought to be, of general concern to those whose thinking has been influenced by Korzybski.

(1) There is a tendency to confuse the structure of language with our traditional western picture of the structure of language. This, of course, is an instance of what Korzybski calls confusing symbols with what is symbolized.

Thus at the beginning of Chapter 2 (p. 8), Rapoport opens his discussion of the topic 'What Are Assertions Made About?' by presenting the two English sentences *I am thirsty* and *Grass is green*. He says 'The word "grass" is the subject of the sentence. . . . We are taught in grammar school that the subject of the sentence is the thing talked about; so it seems proper for us to infer that a man talks about the subject of his sentence.' But such an inference can be the source of confusion, since the person who says *Grass is green* is also talking about something not overtly mentioned in the sentence: himself.

In the passage cited above Rapoport is quoting from Miss Fidditch, who taught you English in grade and high school. Miss Fidditch knew nothing whatsoever about the structure of language from her own observation, but passed on to you a complex assortment of inexact and misleading 'definitions' and 'rules' which she herself had received in much the same authoritarian way. There were numerous ranges of obscurity in Miss Fidditch's discussion of language. Half the time she seemed to be telling us how we *ought* to use language; half the time she seemed to be trying to describe how language *does* work. It was never clear whether at a given moment she was talking about language itself or about writing. Most important for us, all the definitions and all the rules referred indiscriminately to the *structure* of language and the *meanings* of linguistic forms; the two were never sorted out, each being dealt with in terms of the other.[2]

As Rapoport phrases his argument, we are forced to conclude that the source of confusion is not necessarily anything about the structure of language, but rather the ineptitude of the traditional school grammar. It would follow that if we could replace that ill-fitting garment 'traditional school grammar' by a picture of the structure of English more in keeping with the facts — that is, if we could refer to more accurate maps of our language — then Miss Fidditch as a source of semantic aberration would be eliminated.

Until quite recently, traditional school grammar was the best map of language available, but now new and more effective cartographers are at work. There is a branch of cultural anthropology known as linguistics, which has as its subject-matter human language. There is a branch of this latter field known as descriptive linguistics, which has as its task the analysis of a particular lan-

[2]A good general account of the differences between Miss Fidditch and modern linguistics will be found in *Leave Your Language Alone!* by Robert A. Hall, Jr. (Ithaca: Linguistica 1950).

guage as it operates in a given community at a given time, and which has developed surprisingly good tools for the performance of that task. An important feature of the techniques of descriptive linguistics is that the analysis of linguistic structure is undertaken, in the first instance, *without reference to meaning*. A description of English arrived at by these techniques states the structure of utterances and parts of utterances in terms of similarities and differences, in great detail, and can even predict what not-yet-observed utterances are apt to occur; and all of this is done without the slightest implication as to what 'sentences are about' or what the semantic function of a 'subject' is. Clearly, if our map of the structure of a language makes no reference to the meanings of linguistic forms, then it is not going to operate as a source of semantic error. Rapoport's premise — 'the subject of a sentence is the thing talked about' — is not to be found on such a map, and the argument (as stated) falls.

Now, of course, utterances *do* have meaning (at least sometimes), and if the descriptive linguist finds it necessary to ignore meaning, someone else must deal with it. We need an analysis of meaning which is as superior to Miss Fidditch's slipshod efforts along that line as modern descriptive linguistics is superior to her treatment of structure. This, it seems to me, is one of the tasks which general semanticists (though not they alone) are working at. But a discussion of the meaning of linguistic forms has to refer to some map of linguistic structure: choose Miss Fidditch's map, and the results are going to be distorted; choose a better map, and get better results. In other words, general semanticists ought to know linguistics.

With Miss Fidditch out of the picture, do Rapoport's examples lose their force, or is there still something in the structure of languages — whether that structure has been the object of discussion or not — which can mislead? Indeed there is. We have to rephrase our description of the mechanism involved. This is not yet possible in detail, but as a general principle we may perhaps say the following: a speaker of a language (whether he ever heard of 'grammar' or not) will tend to analogize from parallelism of formal structure to parallelism of meaning. He will on occasion argue from, or base action on, such analogy; disputes may result. This is all we need for the *I am thirsty* and *Grass is green* example; the wrong conclusions Rapoport says may be drawn may indeed be drawn, but not quite for the reasons he gives.

Miss Fidditch as a source of error is probably not very important, but there is one tendency in Rapoport's discussions which may reflect just that. On page 10, for example, he presents two people who have said, respectively, *Caviar is a wonderful delicacy* and *I think it tastes awful,* but who obviate an argument when they recognize that neither is talking about caviar, each is talking about himself. I would have said, rather, that their discovery is to the effect that each is talking about caviar *and* himself. Miss Fidditch taught that every sentence has a subject, and only one subject; Rapoport escapes her clutches to the ex-

tent of realizing that the grammatical subject is not necessarily the topic of discussion, but is still slightly in her shadow when he implies that a sentence is nevertheless only about *one* thing.

(2) On page 156 Rapoport says 'Examples of bad use of language can be found in "philosophy." Traditional philosophy has been built on a delusion that it was making true assertions about "reality." It had neither the method for learning about reality (experimental techniques) nor the language to talk about it (mathematics). If only philosophy would leave the assertions about "reality" (events) to science and content itself to inquiries about language (theory of meaning, logic, logical syntax), it could become respectable again.'

Overtly, this somewhat plaintive remark recommends that 'philosophy' should render unto science that which is science's; covertly, it implies that language (in the broad sense) is not susceptible to study by scientific means. Now the latter implication is clearly false. Language (in the narrow sense) is the subject-matter of linguistics, which is a branch of science. Scientific techniques for the examination of other varieties of communicative behavior are currently being developed.[3] Indeed, it seems unfortunate to imply that there exists *any* category of phenomena which cannot be the object of scientific scrutiny.

But in this case, what is left to philosophy? We have a choice of answers: (a) philosophy ceases to have any place; (b) philosophy, like science, has as its subject matter anything whatsoever, but with some fundamental difference in approach; (c) the dichotomy suggested by the pair of terms 'science' and 'philosophy' is a chimera, which ought to be by-passed in any serious discussion. I suspect that the third alternative is correct.

(3) Rapoport has as much difficulty as his predecessors (among general semanticists and otherwise) in formulating any neat description of the difference between human and non-human behavior. On page 15 he says 'For most organisms . . . stimuli are largely energy changes. . . . But man responds in a special way to another class of stimuli — symbols. These special stimuli call forth responses in a way fundamentally different from the nonsymbolic stimuli.' However, 'symbols' are just as much energy-changes as are nonsymbolic stimuli. The difference lies in categories of *response*, not in the energy *versus* non-energy nature of the stimulus. For the same reason, Rapoport's term 'symbolic sense' is highly inappropriate, since it implies a parallel with 'sense of vision,' 'sense of hearing,' and so on. Symbolic behavior involves neither a kind of energy nor a

[3] See, first and foremost, *The Mathematical Theory of Communication*, by Claude E. Shannon and Warren Weaver (Urbana: University of Illinois Press, 1949). This discussion is based primarily on engineering problems arising from modern electronic devices, but has far more general applications.

kind of receptor apparatus in the nervous system other than those involved in the behavior of non-human animals.

'Symbols call forth responses in a way fundamentally different from the nonsymbolic stimuli' — but this way is never made clear; indeed, I am not sure that it can be made clear in the present state of scientific knowledge. Certainly the signal-symbol opposition (pp. 77, 81) does not accomplish this; it translates an obscurity from one terminology to another. There is, however, a good clue on page 81: 'Symbols acquire different meanings through their relations with other symbols: they acquire a meaning in a *context*.' Thus the man, who stops a car only *at a corner* and only for a *particular kind* of red light, differs from the ape who has been taught to drive but who stops wherever he is when he sees *any* red light;[4] and this difference is not so much through a narrower definition of the relevant symbolic stimulus, as by a taking-into-account of a vastly larger amount of the context (symbolic and nonsymbolic) of any given symbol-occurrence. If this is the difference between 'signals' and 'symbols,' then the difference is clearly one of degree, not of kind. Certainly this is a marked difference between human and non-human behavior, whatever other differences there may be.

(4) Rapoport hints several times at a distinction which ought to be made in a clearer way; it would clarify other parts of his discussion. Just as radio transmission of a musical performance involves mapping the output of the orchestra onto radio waves and then back into sound, involving all the problems of fidelity and elimination of various kinds of 'noise,' so communication *via* language involves mapping of experience into language (p. 43). Rapoport notes (p. 44) that the latter is in a sense more difficult, since there is not the vaguest similarity between the physics of the things words mean and the physics or geometry of the words. From this we can distil the notion of two different kinds of meaning-relation, usually found bound up together in one proportion or another: *iconic* and *arbitrary*. A picture of President Truman *means* President Truman by virtue of geometrical similarity, which gives rise to highly similar visual gestalt; this is the iconic mechanism. The word *man* means man not through any geometrical similarity at all, but in spite of the lack of such; this is the arbitrary mechanism. The most far-fetched caricature of President Truman still looks like him; but it is a *caricature* by virtue of certain stylized (arbitrary) differences. Compare a Japanese and western painting of a tree; each looks like a tree, so that there is an iconic element; the two also look different from each other, so that there is an arbitrary element.

With this apparatus, the map-making example on page 82f can be handled more effectively. A map of Manhattan Island *means* Manhattan Island largely

[4] This example is not Rapoport's but Hayakawa's in *Language in Thought and Action* (New York: Harcourt Brace, 1949), p. 24n.

in an iconic way. Not everyone has flown over Manhattan Island high enough to see the whole island at once, but those who have will guarantee the statement just made. It is of course, as yet, a matter of prediction to say that a map of Ohio means Ohio in the same way (that is, 'looks like Ohio'), or to plot the location of lands and seas on a spherical surface (library globes); and the factors which make us confident in assuming the iconic relations here obviously deserve examination. Nevertheless we *do*, quite correctly, feel confident in making the assertion. But when someone thinks of the British Empire as 'eternally pink' (p. 126), his mistake is not that of confusing symbols with things symbolized; it is the case of not having properly sorted out iconic and arbitrary features of a single symbol: the person is interpreting as iconic features which must be taken as arbitrary.

This (and possibly its inverse, i.e., taking as arbitrary features of a symbol which are supposed to be iconic) is an important category of semantic error, which ought not to be lumped indiscriminately with other types under the single description 'confusing symbols with the things they stand for' or 'taking symbols as things.' The man who sneezes in the presence of paper roses seems like a legitimate example of the latter; most of the examples which have been given do not.

(5) It would be a great advantage in discussions such as Rapoport's if a clearer terminological distinction were made between two things. The first of these is the relation between symbolic and nonsymbolic behavior, or, if you will, communicative and noncommunicative behavior; the difference is probably one of degree rather than kind, and I shall call this scale of contrasts the *theory-practice* scale. The second is the relation between relatively implicit behavior of an organism (inside his skin, or even inside his central nervous system) *versus* relatively explicit behavior, observable by others; this is also a matter of degree, and I shall call the scale of contrasts the *thought-action* scale. These two scales intersect at right angles, producing four quadrants. I can 'think' a word to myself or I can say it out loud; both acts are 'theory' rather than 'practice,' but the former is 'thought,' the latter 'action.' I can 'think' steering a car over a familiar route, or I can drive it; both acts are 'practice' rather than 'theory,' but once again the former is 'thought,' the latter 'action.' The Korzybskian terminology is unfortunate because it lumps events in three of the quadrants (theory-thought, theory-action, and practice-thought) together under one rubric 'symbolic,' and leaves only that in the fourth quadrant as 'nonsymbolic.' It thus becomes difficult to say some things clearly.

For example, the phrases 'the internalized structure of our language' (p. 166) refers only rather awkwardly to what is meant: 'the conclusions we are inclined to jump to,' both because of the impact (from our participation in life with others) of the meanings of symbols, and also from a similar impact from

nonsymbolic aspects of life (that is, the impact of experience coming from outside into *both* 'thought' quadrants). 'All of us think in terms of language' (p. 89) is not a justified conclusion from the argument which immediately precedes it, but a claim that 'all of us think (i.e., carry on inside) in terms of what we do, or what is done to us, overtly' is a valid conclusion. On page 30: 'We see an invisible shop window. We think "Maybe there is no glass there." It is this *assertion* (verbalization of experience) that we set out to test when we stretch out our hand to convince ourselves that the glass is there.' There is no guarantee that the experience is verbalized before a test is run off on it. It is quite possible that speechless animals can be observed to perform similar actions. All of these formulations reflect the inadequacy of a one-dimensional treatment (symbolic-nonsymbolic) of an essentially two-dimensional array of facts (thought-action and theory-practice).

THE KORZYBSKIAN terminology even makes it difficult to state the fundamental outlines of Korzybskian theory with clarity, since the terminology, as it were, assumes a particular answer to the very problem which has to be solved.[5] In our nonce terminology, we can give those outlines. The theory-practice scale is trivial or nonexistent for non-humans. Its existence among humans makes possible *collective* formulation of observations and testing of hypotheses, largely or completely impossible for non-human animals. The theory-practice axis is for *communities* of human beings what the thought-action axis is for an animal or a lone human. Because a human being lives in society, however, he has within himself both scales of contrast even when, temporarily, he is alone. Thus even the lone human has a great advantage over the animal.

Accurate adjustment of theory to practice is science. Accurate adjustment of thought to action is sanity. Now comes the Korzybskian hypothesis: the pattern of relationships along the theory-practice axis, as a given individual acquires it from the society in which he lives, tends to shape the pattern of relationships along that individual's thought-action axis into an image of itself (Korzybski's 'internalization of language'). If this is true — and it is a hypothesis susceptible

[5] From the lunatic fringe of our collective field of interest has come the suggestion that we should devise a 'language' in which it is impossible to make false-to-fact statements. Quite apart from the impossibility of devising such a 'language,' it would be totally useless for scientific purposes. To qualify as a scientific hypothesis, a statement must contain, at least implicitly, specifications as to the conditions under which it is to be *rejected*. Thus the 'language' we use must provide machinery for stating things that are *not* the case — preferably a multiplicity of them, so that the phrasing we eventually chose (as a *pro tem.* hypothesis) stands out as sharply as possible against those which have been rejected.

A 'language' — that is, a terminology — in which it is difficult or impossible to make *scientifically meaningless* (= not provable and not disprovable) assertions is a more realistic aim. But it will come only piecemeal, as one byproduct of continuous scientific activity.

of empirical investigation — then science makes for sanity, lack of science for non-sanity; the methods of science take on the status of social therapy, and from them are derivable techniques of individual therapy; and, finally, Rapoport's thesis that science generates its own ethic is necessarily correct.

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It Did Happen Here

AMERICANS BETRAYED: POLITICS AND THE JAPANESE EVACUATION, by Morton Grodzins. Chicago: University of Chicago Press, 1949. 445 pp. $5.

IT HAS BECOME somewhat fashionable, at least among those who are not entirely convinced that World War II was a clear-cut conflict between the forces of righteousness and the forces of evil, to refer to the relocation centers in which American citizens and others of Japanese ancestry spent the war years as 'concentration camps.' A little sardonic laugh often accompanies such reference, which suggests that the speaker considers himself sophisticated enough not to be fooled by the label 'relocation center.' The assumption is implied that we, just like the Nazis, put certain groups into concentration camps, but that we simply called the camps by a politer name in order to gratify our own easily beguiled sense of righteousness.

A semantic analysis of the relocation policy, however, calls for considerably more insight than a mere insistence on 'calling things by their right names.' As a matter of fact, in *Americans Betrayed,* Morton Grodzins' fascinating study of the origins of this policy, we discover that these camps *were* called by their 'right names,' so that this is clearly not the problem at all. Many of the pressure groups which were influential in bringing about evacuation of the Japanese—Americans and aliens alike—from the West Coast explicitly advocated placing them in 'concentration camps.' Even the commanding general of the Western Defense Command, who finally made the fateful decision, used this ominous term.

Grodzins' book does not deal with the life of the Japanese relocation centers (he credits the War Relocation Authority with having done 'a courageous and humane wartime job'), but with the 'process of decision' by which the evacuation of Japanese-Americans from the West Coast came to be adopted as a policy, 'from its original public discussion, through administrative consideration and adoption, to legislative indorsement and judicial approval.'

With a wealth of quantitative material he traces the growth of pressure for evacuation, which, after an initial period of post-Pearl Harbor good will, rose to a crescendo of demands for group-treatment at the time the policy was actually being decided upon, and which remained unabated after the evacuation order

had been given. Among private citizens and interest groups advocating evacuation, among Army officials charged with the defense of the West Coast, among Congressional delegations from that region, and among the members of the Congressional committees concerned with the enforcement legislation which followed the executive order, the 'a Jap's a Jap' pattern of thinking prevailed.

Despite its otherwise excellent qualities, Grodzins' book is rather unconvincing in its evaluation of General DeWitt's thought processes. The general's testimony before a subcommittee of the House Naval Affairs Committee on April 13, 1943, included the statement: 'It makes no difference whether he is an American citizen, he is still a Japanese.' [1] (pp. 282-283) Grodzins remarks: 'This extraordinary statement followed not only the essential racial doctrine of the Third Reich but almost the same figure and almost the same words of Adolf Hitler himself.' And he quotes an analogous passage from *Mein Kampf*. The weakness of the 'a Jap's a Jap' mechanism could have been adequately demonstrated without resorting to guilt by association, and *Americans Betrayed* would have been a better book without this digression.

DEMANDS for evacuation came from pressure groups of all types, from those whose existence was for the specific purpose of opposing assimilation of Orientals, from various veterans' organizations, and from economic pressure groups. By contrast, 'No groups of comparable influence have worked for the cause of integrating Japanese into American life.' (p. 12) 'Why?' the student of general semantics surely must ask. Though Grodzins' book contains a mine of material which supports several of the hypotheses of general semantics, it does also pose this important challenge: are the mental short-circuits which general semantics studies intrinsically and inevitably negative in their influence? That is, must stereotyping, thinking in identities, confusing levels of abstraction, etc., always be associated with broadly anti-social ends? The book offers no answer to these questions, but leaves them unanswered as a challenge to general semanticists—a challenge which must be faced because the mechanisms are clearly so powerful. As Grodzins says:

> It is important to recognize the essentially emotional character of much of the agitation carried on against the Japanese. But to remove the factual basis from animosity is not to remove animosity. The very emotionalism with which the Japanese issue was charged lent strength to the anti-Japanese groups. (p. 15)

[1] General DeWitt's official statement to the Secretary of War, recommending the evacuation policy, contained the assertion, 'The Japanese race is an enemy race . . .' (Grodzins, p. 281.) During a debate in Congress on Feb. 18, 1942, Congressman Rankin of Mississippi said, 'Once a Jap, always a Jap! You cannot change him. You cannot make a silk purse out of a sow's ear.' (p. 86) Grodzins repeatedly introduces quotations to show how these, and other men in high position, as well as writers of letters-to-the-editor, etc., adhered to the 'a Jap's a Jap' position.

Why to the *anti*-Japanese groups and not to the *pro*-Japanese, or on a higher level of abstraction, the pro-civil-rights, groups? It is all very well to point out the fallacies in our habitual thought patterns, and to admit that those patterns are powerful even when false, but it still remains for us to find the factor or factors (other than truth or falsity) which *do* determine which patterns will prevail in competition. If the potency of an idea is not highly correlated with factuality, what *is* it highly correlated with? According to Grodzins' study the answer would seem to be 'emotionality.' But what makes one idea 'emotional' and another not?

A partial explanation is offered by Grodzins, when he points out that it is not necessary to

> . . . impute hypocrisy to those who urged evacuation as a measure of national defense but who simultaneously satisfied other desires. The war provided the unique situation whereby patriotism (*i.e.*, a desire to protect the West Coast from the enemy) could become parallel with economic, racial, and political considerations. (p. 60)

Can we hypothesize, then, that an idea will vary in potency in direct relation to the number of interests for which it serves as an abstraction? This hypothesis is at least in accord with the further fact, according to Grodzins, that 'In denying the racial basis of sabotage danger, opponents of evacuation suffered not from a lack of facts but from the simple lack of a believing audience.' (p. 205)

That there was a marked difference in kind between the sort of arguments advanced against the Japanese and those advanced on behalf of them was discovered by Grodzins in studying the results of a mail survey of local officials in the three coastal states. Statements classified as unfavorable frequently made sweeping generalizations about Japanese-Americans. In contrast, 'the "favorable" viewpoints were couched in restrained tones. They were characterized by no condemnation of the racial basis of evacuation. Rather, in almost every case, favorable comments were based on the high regard held for one or two [Japanese] residents of long standing.' (pp. 116-117)

GENERAL semanticists also face a challenge in explaining the *selective aspect* of the mechanism of identification. Grodzins' book shows how powerful the 'a Jap's a Jap' notion can be [2] but it also shows that this mechanism had no effect on attitudes toward Americans and aliens of German and Italian extraction.

[2] If the population studied by Grodzins is regarded as the experimental group, those studied by Andrew Lind in *Hawaii's Japanese,* and Alexander Leighton in *The Governing of Men* might be considered the control groups—where men in positions of authority were not hypnotized into thinking 'a Jap's a Jap' but instead recognized that 'Jap_1 is not Jap_2.' The contrast between what happened under the two patterns of thought can be attributed in part to the influence of the respective thought mechanisms. [The above-mentioned books are reviewed by J. Paul in *ETC.* 7.222 (Spring 1950).]

> Germans and Italians, as well as Japanese, were distributed in strategic areas; held dual citizenship; had shown in the pre-war years a regard for their country of origin; had their language schools, their business and fraternal organizations, their visits to the old country; and had retained their Old World culture patterns. Yet army authorities did not impugn the loyalty of resident Germans and Italians for these reasons. On the contrary, almost simultaneously with the evacuation of Japanese (both aliens and United States citizens) from the interior area of California, military restrictions against Germans and Italian aliens were materially relaxed even in the coastal zones. (p. 297)

Why didn't West Coasters also insist 'a German's a German' or 'an Italian's an Italian'? The explanation for this contrast, according to Grodzins, is to be found in 'the combination of (1) historic regional animosities toward resident Japanese and (2) a crisis situation which made the full exploitation of those animosities not only possible but uniquely patriotic.' (p. 173)

One other plausible explanatory element is mentioned, which is of interest to general semanticists:

> The German enemy (Nazi) and the Italian enemy (Fascist) could be verbally distinguished from Germans and Italians in the United States, but no such convenient nomenclature existed for the Japanese. In most public discussions, both citizens and enemies were 'Japs.' (p. 202)

This might well raise the general problem (which Grodzins touches upon in the first Appendix) of the effect of newspaper headline-writing on the level of semantic sophistication of the newspaper-reading public. 'Jap' was a conveniently short word which found its way into headlines far more often than the cumbersome phrase 'Americans of Japanese ancestry.' The mere fact that it did not distinguish between U. S. citizens, alien Japanese in America, and Japanese in Japan, was, of course, no deterrent to the headline writer.

In the same manner, perhaps, 'Red' is today a convenient headline word. Whatever may be the relationship on the sub-verbal level between Communist parties throughout the world and the Kremlin in Moscow, the use of this headline word for all Communists could hardly have done otherwise than to create at the verbal level the impression in the public mind of a world organization of 'Reds.' This is all well and good if the verbal impression checks with the sub-verbal facts, as perhaps it does, but how many hurried headline-readers are aware of the process of abstraction and are in the habit of checking to lower levels?

The relocation policy specifically involved the confusion of abstraction levels. 'In almost every discussion, proponents of the movement urged loyal Japanese to cooperate and branded as "disloyal" those who refused. This line of reasoning was extended and used to silence non-Japanese.' (p. 203) This should be a warning: if another situation arises in which the same kind of forces are at play —patriotic, regional, economic, racist, etc.—in the same kind of relationship, the general semanticist who tries to say 'Jap_1 is not Jap_2' (or whatever the

variable involved) will find himself charged with disloyalty!

Over and above its semantic significance, this book should be of intense interest to social scientists generally, for it has become fashionable in many quarters, lay and academic, to attribute much of the current social unadjustment to an alleged 'lag' of the social behind the physical sciences.[3] We are in for trouble, it is asserted, until the social sciences 'catch up' with the physical sciences. Grodzins points out that 'military officers did not in a single instance rely on the large mass of scientific materials that had been gathered about American Japanese.' (pp. 301-302) Not only were scientific studies about the minority to be dealt with not consulted, but

> . . . no use was made of the techniques that existed to make an accurate estimate of the actual state of public opinion on the west Coast. Relatively exact data could have been collected on short notice with scientific survey techniques; and there were available within the federal government both the technical personnel and the administrative machinery to undertake the collection and interpretation of this information. (p. 366)

These considerations should make it abundantly clear that the lag is not entirely between the degrees of development of the two classes of sciences, but is at least in appreciable part to be found in the absence of any public habit of turning to the social scientists for necessary information. Information was available on the characteristics of Japanese-Americans. 'But the library shelves were undisturbed, and the Japanese specialists were not consulted.' (p. 367) Information could have been made available on the actual state of relevant public opinion, but only one piddling little attempt at a poll on the subject was made, consisting of 102 interviews in a handful of California localities, during the last week in January, 1942. Perhaps there is more need for the people and the makers of public policy to catch up with social scientists than for the social scientists to catch up with physical scientists!

GRODZINS' *Americans Betrayed* is not just a study of an historical incident; it is a warning for the future. We may not again evacuate as a group people of a particular ancestry as we did in 1942. But few are likely to deny that history may repeat itself in more abstract terms, and the possibility of a single repetition demands, because of its serious implications for American traditions, our careful consideration. Grodzins emphasizes that:

> One and only one argument in support of the measure [to provide court enforcement of evacuation policies] was used: the bill was recommended and considered essential by military authorities. This statement appeared in both committee hearings, in both committee reports, and in discussions before both houses. Yet no single congressman or senator

[3] See Hornell Hart, 'Atomic Cultural Lag: I. The Value Frame,' *Sociology and Social Research*, 32:768-776 for a list of prominent sociologists and other writers who advance this thesis, and for a critical evaluation of it.

> investigated its basis or reasonableness, just as no single member questioned the military injunction that mass evacuation was essential to the national defense. (p. 347)
>
> Time after time, those who expressed a moral or political repugnance to treating groups en masse qualified their statements by saying 'unless required by the military'. . . . to some 'military necessity' was inscrutable and therefore incontrovertible. (p. 206)

'Military necessity' was thus a high order abstraction which virtually everybody was willing and (seemingly) almost eager to pay homage to. It is not impossible that this same high level abstraction may soon again occupy that prestigeful position. When it does, or when any other concept occupies such a position, we must, if we are to have learned anything practical from *Americans Betrayed,* exercise extreme caution in regard to the sort of procedures, policies, attitudes, and goals which we allow ourselves to adhere to under the prestige of such a symbol.

Americans Betrayed should be carefully read by every serious student of general semantics and of social science, both as a valuable source of data and as a challenging stimulus to further research and thought. As one whose opinion as to the wisdom of the relocation policy was changed by reading it, I can also recommend the book to anyone who 'likes to be shown.'

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Microtonality Revisited

GENESIS OF A MUSIC, by Harry Partch. Madison: University of Wisconsin Press, 1949. 362 pp. $10.

THIS REMARKABLE book will very likely not get the wide readership it deserves. To the lay reader's quick skimming, it may seem formidably difficult; the careless reader may badly misread its provocative statements and skip its ratio presentations; the conventional will be distressed by the destruction of dearly cherished musical notions. It will be simplest for our musical industrialists, anxious to preserve the 'industry,' to deride it as impractical radicalism. The total critical attitude may well prove to be that ironically castigated by Rousseau: 'Whatever he does, he is bound to be wrong for two reasons, the one because he is an inventor; the other, because he has to do with musicians.' Attentive reading, however, should rank this book among the most significant musical documents of many years.

Were every theory, every mathematical computation, every musical judgment proved wrong, the book would still be significant. For Mr. Partch's treatise stimulates basic thinking on the nature of music, the foundations and theorems on which it is constructed, its future directions.

Roughly, three major arguments emerge. First, there is the demand that as much attention be given to what Partch calls Corporeal music, as is now given to the doctrines of the Abstract. (This is rather a request for *de jure* acceptance; actually, both partially and strictly Corporeal creations do exist, to no inconsiderable degree, in 'popular' music).

Second, he asks that we expand the number and types of our musical instruments.

Third, he urges that we give consideration to methods of adjusting present scales and formulating new ones, with the development of appropriate notations.

Partch's statements about Corporeality are pleasant in this period of absolutist criticism. Examining plinths, acknowledging overlapping, a splendid outline of inherent possibilities in Corporeal music is constructed. The author does not propose 'an ecumenical prohibition of the Abstract. There is nothing wrong with Abstract music, as such . . . ' He does re-emphasize, though, that it is not the sole respectable vehicle for composition.

In the remarks on Corporeality, I find certain omissions, perhaps necessitated by the demands of brevity and clarity. The origin of music is open: phonists find it in the sounds of nature; rhythmists in nature's pulsations; rhetoricians in the confluence of words. One might have wished for a fuller discussion of timbre and the vibration range of song. Likewise, I find Partch's discussion of phonetics too brief, although he is one of few composers who acknowledge the science. There is need for more discussion of words and word-relationships—phonemics, jointures, speech rhythms, and distortion limits. Since phoneticians have paid far too little attention to the problems of song, *Sprechgesang,* and the melodic elements of speech, Partch's comments on these topics would have been invaluable. These omissions, however, do not in the least diminish the value of Partch's insight into the fundamental motivations underlying Corporeality in music: its concern with individual expression, delineation of meaning, and the integration of verbalization, vocalization, melodic line, harmony, and rhythm.

WE ARE at present restricted almost entirely to Equal Temperament (the familiar twelve tones of the piano keyboard) and a familiar group of instruments, little changed over hundreds of years. The notion is still widely held that the best music is an abstract, 'pure' music, with little correlation to germane human experiences and literary and linguistic experience. Other musics (and there are many) are often labelled exotic, archaic, anachronistic. There is little consideration of them in esthetic or performing terms. Partch, however, starts a different assumption:

> Music, 'good' or 'not good,' has only two ingredients that might be called God-given: the capacity of a body to vibrate and produce sound and the mechanism of the human ear that registers it. These two ingre-

> dients can be studied and analyzed, but they cannot be changed; they are the comparative constants. All else in the art of music . . . was created by man or is implicit in human acts and is therefore subject to the fiercest scrutiny—and ultimately to approval, indifference, or contempt. In other words, all else is subject to change . . .
>
> In a healthy culture, differing musical philosophies would be coexistent, not mutually exclusive, and they would build from Archean granite . . .
>
> Moreover, with our highly developed reproducing media, there can be no conceivable objection to the coexistence of different intonational philosophies, allowing the individual composer the choice of anything current or anything he can concoct.

Yet the facilities for such explorations are virtually non-existent. The academies remain content with Equal Temperament and the architectonics of virtuosity. Our theorists are mostly bound by the tyranny of the keyboard. The equipment and training for other than one source scale and one harmonic system are not provided.[1] Therefore, the composer in search of new musical resources must generally choose isolation (losing audiences, and the chance for much group research), a reputation as 'esoteric' and difficult, and little remuneration.

Admittedly, however well they know the academic formulae, many, if not most, composers do not think in these theoretical terms. Anxious to create, they ignore the nature of the materials, content with whatever is at hand. They eke out with mud and straw, though cement and steel could be manufactured. They are willing to suffer inefficient nomenclature and notation. Our *avantgarde* innovators tend to work unsystematically: their 'prepared pianos' and other retoolings, and their percussion experiments are stylistic facets, sometimes merely affectations. The more solid experimenters (*e.g.* Barth, Haba) have little renown and less effect.

In this connection, Partch's remarks on recordings are especially perspicacious. The phonograph, capable of projecting instantly the composer's wishes, can give wide circulation to new or complex systems. That it does not do so in fact is regrettably apparent. Recordings make possible a new type of musical multiplicity. Presuming the intelligent, sympathetic, and involved auditor, records can demonstrate root problems, display total reassessments aurally, document many now esoteric musics (*e.g.* Balinese *gamelangs,* Indian *ragas,* etc.) and, eventually, give scope to every aspect of the musical imagination.

THE development of new instruments, of course, dovetails very closely with the development of new scales.

> Composers can 'think' only in Equal Temperament for just one reason: because it is all they have got to think in. Music systems are made valid—

[1] Mr. Partch has recently received a Guggenheim Fellowship to do research on electronic instruments at the University of California. He has previously held the Guggenheim and Wisconsin Alumni Research Foundation Fellowships for research.

> and workable—by significant music . . . But to produce significant music in Just Intonation we must have instruments, and instruments are no small problem.

I cannot imagine anyone reading this book who will not find himself eager to experiment in the construction of new instruments, who will not be pressed into experimentation by the stimulating problems of ratios. Close attention to the tables reveals exciting potentialities. It is to be hoped that this personal curiosity, to hear and build unfamiliar scales, to manufacture the necessary devices, will be widespread. In direct relation to Corporeal doctrines (and like much of the *Bauhaus* discipline), one is inspired to individual creativity which, if nothing else, leads to greater and more intense appreciation and understanding.

Within the compass of the octave, we currently accept twelve more-or-less equally divided tones. Within any given octave many more tones may be placed in a variety of ways. Within Partch's octave there are 43 tones, in a just temperament whereby the separating intervals are not even. This neither precludes consonance nor a clear separation between tones, and allows for modulation.

The capacity of the ear to discriminate is far greater than the demands made upon it. We hear far more than we appreciate. Easily capable of making the adjustments Equal Temperament forces upon it, the ear can readily differentiate the 43 tones of Just Intonation, and a good deal more. We have long known that it can distinguish 'quarter' tones, 'eighth' and 'sixteenth' tones, and other microtonal intervals. Its ability to assess subtleties of poor intonation, the fractional shifts of speech, the sharp or flat A of orchestral tuning, and other similar phonic phenomena, are evidences of a sensitivity equal to or greater than our rhythmic sensitivity. This aural sensitivity is unused and unrecognized because of the absence of musical idioms richer than those limited by the 12-tone system. Carl Seashore wrote that the 12 tones of the Equal Temperament scale are the smallest intervals the 'unselected average' can enjoy. In view of the existence of much finer gradations in the musics of other cultures (*e.g.*, Indian), one may well doubt this statement. The 43-tone scale is not posited on the nearest distinctive audible intervals, but on a logical ratio series which can hardly be summarized here. It is, importantly, capable of great refinements of consonance. However, no scale is valuable as such. Its value is dependent upon the music set in it.

> Furthermore, music is not a fantasy of chromaticism. A scale is a source of materials, from which chords and melodies are drawn, and in which the scale as a scale appears only occasionally, if at all.
>
> The intonational thesis of monophony is four-fold: first, the expression of true or just intervals as an expansion from unity; second, augmentation of the chromatic potential; third, expansion of tonality; fourth, attainment of variety in the quality of tonality. Regarded in this light, the forty-three degrees as a tonal succession need no justification beyond the

statement that each separate degree is easily perceived in the common registers of music.

From this scale, it is plain that one will get intensive melodic and harmonic variety, new resources, and richer counterpoint. Expansion and diversity of form follow and, I think likely, rhythmic expansion as well.

FINALLY, Mr. Partch presents several other possible areas of creative activity and interest besides his own. Of these, Schoenberg's destruction of the tonal center—the dodecatonal (or dodecaphonic) method—and Joseph Yasser's 'evolved tonality,' a 19-tone equally tempered scale, are closest to current usage.

The final assay of Partch's views is not conditional upon his composing techniques.[2] In one sense, they are even irrelevant. We must recognize the importance of tonal exploration to dynamic creation. The book is addressed to those 'with a musically creative attitude': composers and theorists, of course, but, I think above all, 'anyone, even without a knowledge of ordinary musical theory, who has this creative attitude.'

The Futurists recommended a contemporary music which would recreate the tones of the jangling, hooting, bellowing, snorting, clashing world, a music of klaxons, motors, steamwhistles, backfire, foghorns, and sheer noise. Its ultimate end was a symbolic depiction of the aural world. This blustering, naive bombast has no echo in the lucid, erudite, intelligent pages of Partch. The reading of *Genesis of a Music*—whatever one's final opinion of it—is a deeply provocative intellectual experience.

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Toward Understanding Understanding

THE NATURE AND TECHNIQUE OF UNDERSTANDING: SOME FUNDAMENTALS OF SEMANTICS, by Hugh Woodworth. Vancouver, B.C.: The Wrigley Printing Company Limited, 1949. 142 pp.

THIS is a most curious book, which can neither be casually dismissed as trivial and unimportant, nor strongly recommended as a contribution to the literature of 'semantics.' It is a difficult book to evaluate. I am not familiar with the author's name, his background, or his other works, if any; and there is nothing in or on the book which would help to compensate for this lack of information. One is usually willing to read a book on its own merits, without too much worry about the author's status as 'authority.' *The Nature and Techniques*

[2] Recordings of Partch's own *U. S. Highball* (Gilsonophone) may be obtained from Dr. Warren Gilson, University of Wisconsin, Madison, Wisconsin. Gilsonophone plans the release of other Partch recordings at a later date.

of Understanding is oddly difficult to read in this way, and as a result the experience of reading the book is a slightly grotesque one.

Mr. Woodworth seems not so much interested in interpersonal communication as in problem-solving. His book might be described as an attempt to characterize the 'mechanisms' of 'understanding,' the implements of thought. He devotes a chapter to an analysis of 'consciousness,' which term he defines as awareness of experience, and equates roughly with 'mind.' Another chapter discusses 'feelings, freelings and sensations,' *i.e.* sense impressions and their 'emotional' concomitants. 'Meaning' is then defined as 'that portion or area of consciousness which is called up when the word is seen or heard.' Meanings may be 'verbal' (minimum) or 'extended' ('dwelling on' a word or statement). 'Understanding' is then discussed as 'still more extended meaning' and is eventually described as a kind of reliving of experience by means of language.

While this is by no means an adequate summary of Woodworth's discussion, it is perhaps a reasonable sample of the kind of thing he attempts. Several comments may be made of his work. First, it is clear that Woodworth's 'mechanisms' of thought are fictions — just as the 'mind,' the 'will,' the 'super-ego,' the 'emotions,' are fictions. The question is not, does man have a consciousness? but rather, if we postulate a 'consciousness,' what then? What can we accomplish? In Woodworth's case, I am not too sure. His theory of 'understanding' is clear, fairly reasonable in the light of any evidence I know, and generally convincing. But what results from it? The author himself seems to go on the assumptions that this is *the* explanation of the phenomenon of 'understanding,' and if you 'understand' it, your thought processes will become more effective. I am not so sure. The author's insights, while acceptable enough, do not seem to me as brilliant and penetrating as all that.

Second, Woodworth's discussion of the fictions seems extremely abstract, although he attempts throughout his book to refer frequently to 'common' human experience. His focus is very much on the terms themselves, and he seems to relate abstraction to abstraction very freely indeed — for example, his relation of 'mind' to 'experience' to 'consciousness' to 'awareness.'

Finally, the book is completely undocumented save by reference to 'common' (and hence, hypothetical) human experience. There is no mention made in the book of any research, by Woodworth or anyone else, or of any verifiable data of any kind. As a result, the author places himself in a curious position. He appears before the reader and says, in effect, 'Let me tell you an interesting story about how we think.' He does so; but the reader (this reader, anyway) is left with the feeling that it is, after all, only a story, wondering just how seriously to take the whole affair.

I do not mean to suggest by these comments that I think Woodworth's book useless. It may, indeed, be vastly more useful than I am inclined to believe at this moment. In any case, one can never afford to discard or ignore

new theories of 'meaning.' As far as students of general semantics are concerned, they would do well to read Woodworth and, as they find his formulations useful and stimulating, add them to the growing body of hypotheses that are constantly being tested and revised in this field.

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Meanings and Poems

READING POETRY, by Fred B. Millett. New York: Harper and Brothers, 1950. 254 pp. $2.

PROFESSOR Millett approaches the problem of teaching a beginning literature course in college with the aim of teaching the technique of reading poetry analytically — certainly, from the point of view of a literature course, a most fundamental and important aim. The historical approach — from Beowulf to Hardy, or from Chaucer to Frost — is a sound one, provided the students can be expected to have a grasp of the nature of the abstractions upon which history is based, an understanding of the dangers of using these abstractions, and a technique for reading poetry analytically. If these requirements can be presupposed, the historical method is valuable in integrating a large fund of texts and deriving abstractions from them; certainly anything approaching a 'complete' reading of a poem requires an awareness of its historical background. But since we often cannot presuppose these requirements even among seniors in college, it is obviously dangerous to expect any of them from students just out of high-school. A sound remedy, then, is to begin as Millett does.

Reading Poetry is divided into four parts — an introductory section on 'The Elements of Poetry,' discussing structure, rhythm, imagery, and sound patterns; then a central section containing fifty poems by poets from the old English ballad writers to Shapiro and Spenser, arranged not chronologically, but into five sections: narration, characterization, description, feeling and emotion, and philosophical poems; finally, two appendices, 'The Study of Literature,' justifying the analytical approach, and 'The Values of Poetry,' discussing the reader's reaction to the technical, psychological, and ideational aspects of poetry.

Millett's book invites comparison with Brooks and Warren's *Understanding Poetry,* which appeared twelve years ago. The greatest difference between the two books is that Millett limits his collection to fifty poems, while Brooks and Warren include over two hundred. There are advantages to each — obvious advantages in having more than two hundred poems to choose from rather than fifty; but the larger number of poems may be more easily misused to teach extensive rather than intensive reading, thus permitting a disintegration into superficiality. It is possible that the stringency of the smaller number of poems will prevent this.

Millett includes no such lengthy analyses of the poems as do Brooks and Warren; on the other hand, every poem in Millett's book is followed by a short series of searching (and usually unpedantic) questions while Brooks and Warren sometimes ask of a poem only 'How does the first stanza set the tone?' Millett has apparently gone to some pains to make his questions stimulating rather than disciplinary, and his frequent suffixing of his questions with the phrase, 'Why or why not?' places a healthy emphasis on having *some* understandable and understood attitude toward the poems, rather than on having a 'right' attitude. Millett's emphasis has probably been influenced by the 'new criticism,' particularly in the intensive examination of poetic imagery, but he avoids the narrowness of Brooks and Warren's stress on irony and paradox, adapting his analysis to the poems, rather than the poems to his analysis. Like Brooks and Warren's book, Millett's *Reading Poetry* should be an effective tool in teaching a college literature course based not on backgrounds and biographies, but on an understanding of the texts of the literature itself.

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The Experiencing of Poems

PREFACE TO POETRY, by Charles W. Cooper, in consultation with John Holmes. New York: Harcourt, Brace, 1946. 737 pp. $3.

PREFACE TO POETRY is one of at least half a dozen books of the last decade designed to aid the student in deriving a satisfying experience from reading poems. The core of the book is an application of Chapter 16 ('The Analysis of a Poem') of I. A. Richards' *Principles of Literary Criticism,* with other materials drawn from later chapters. The intent of the book is to help students go through the proper steps in reacting sensitively to a poem and in achieving poetic experience. In this it realizes what it sets out to do as no other book has done. Each process is detailed so that the student can follow the steps; there are no dull professional platitudes which are meaningless for the student. Professor Cooper's purpose was to write a textbook for students, not for the professors, as is so often done.

Chapters subsequent to Part Two, 'Anatomy of the Poem-Experience,' take up the questions of truth and greatness in poetry, poetic types and moods, choral reading, etc., to make the book a coverage of the whole of the subject of poetry for undergraduate purposes. Here, naturally, it is less indebted to Richards, but the author brings a wealth of teaching experience and sympathy to his task. He is willing to utilize any aid to the student's understanding of poetry, whether materials from the historical investigator of literature or recordings of the voices of the poets. He does not, therefore, fall into the trap of the 'esthetic' critic who, though often using scholarship, denies its use to the student. Pro-

fessor Cooper is eminently sane, cosmopolitan, and discreet in making broad use of all that is pertinent to reading poetry.

At the end of each chapter, there are suggestions for further study. These include investigative assignments and, to carry out the design of the book, assignments directing the student to read and 'experience' particular poems as further means of his development. Generally these are well conceived and directed.

ONE OR TWO caviling questions might be raised about minute items in *Preface to Poetry*. For example, Professor Cooper divides language functions into information-giving, problem-solving, and the 'art-function' of 'emotional adjustment.' This last is further explained as creating in the reader 'an experience of a special sort.' Although he does further identify and limit the 'special sort' of experience, the reader feels that the author becomes a little wistful and appeals to the students' sympathy rather than giving adequate help. Furthermore, one cannot be sure — the idea is not in Richards' volume or in other sources — that the experiences that result from language stimuli are so much an either-or proposition or that utterances may be so exactly divided. Richards himself recognizes only scientific or factual language and emotive language, and Sapir rightly observes that all verbal symbols carry a modicum of emotional stimulus, however 'objective' or technical they may be. Cooper also identifies only information-giving language as *communication,* a limitation of the term not general, I believe, among linguists and semanticists.

A further comment upon both Cooper's and Richards' treatment of the topic of seeing the poem on the page is warranted. Not only, as they say, does the appearance of the poem provide the reader's impression of the poem, but it stimulates also his first reaction. For the beginner, especially, this impression may be quite incidental, or even contrary, to a final judgment. The reader needs, therefore, sometimes not just to utilize this impression, as Cooper suggests, but he must be ready to divorce it from his final conclusion.

These comments do not imply a serious criticism of the book. Any teacher is likely to have his own individual views and to impose them on his students in opposition to a text terminology and usage. One additional criticism seems to me more serious. Professor Cooper creates the feeling that he is writing down to students. Juniors and seniors at this university, among whom were many veterans, felt this acutely, although younger students would doubtless feel it less.

Otherwise, it is unquestionably the best book to date on reading poetry. Although other books since have dared to use less of the semanticist's contribution to the study of literature than Professor Cooper's has, they have thereby risked the charge of archaism in a comparison with his.

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A Guide to Linguistic Practice

LEAVE YOUR LANGUAGE ALONE! by Robert A. Hall, Jr., Ithaca, N. Y.: Linguistica, 1950.

PROFESSOR HALL'S *Leave Your Language Alone!* is, according to his prefatory statement, 'intended as a brief, popular discussion of a number of problems connected with language,' and as such it is highly successful. It is to be regretted that its teachings will probably not reach those who need them most and that it will inevitably be misunderstood by those who read as they run. Hall's definition of 'good' language as 'language which gets the desired effect with the least friction and difficulty for its user' has, in fact, already been misinterpreted by the two-valued believers in 'correct' and 'incorrect' English to mean that 'anything goes' and that standards of usage do not exist, although Hall makes it perfectly clear that there are indeed standards, which vary according to social group and occasion, among other factors. He cites the sad case of a friend who, when he went to work in a shipyard during the war, was regarded as a snob until he substituted *them things* for *those things* in his speech. In the group in which he temporarily found himself, *those things* was definitely not 'good' language, for it engendered the same sort of friction that *them things* would engender in what are regarded as politer circles. The avoidance of such a construction as *them things* has, because of the power of fashion and not because of any inherent 'badness,' become a hallmark of social caste. But Hall neither recommends nor condemns, for to do so is no part of the job of the linguist: he is content merely to record and interpret the facts.

In a democracy such as ours, where equality is prized far beyond liberty, we had all best think pretty much alike about most things; and those who think alike usually express their thoughts pretty much alike. It is true that a certain freedom is allowed within carefully circumscribed limits; but if one's patriotism is not to be suspect, one had best subscribe to the national hagiolatry and never question either the sense of humor of our popular radio comedians or the linguistic authority of those who have told us that 'it is I' is good English and 'it is me' bad English — the tribe of *soi-distant* grammarians who write the handbooks of 'correct English' and prepare the lists of 'grammatical errors' and of words which practically everybody 'mispronounces.' (In moments of unusual daring, some of us may wonder how they know all the things they do. Who tells them how words ought to be pronounced, how sentences ought to be constructed? Is there indeed a linguistic Jahveh who has elected certain choice souls to whom He may reveal the truths of language?)

In view of our depressing national passion for standardization, it is a happy circumstance that, despite certain regional variations which are tolerated (if indeed grudgingly—witness the attitude of the average Midwesterner towards the

speech of a Bostonian), one of the most noteworthy characteristics of American English is it uniformity. Diversitarianism, linguistic as well as moral, has always been repugnant to the great mass of our people. Consequently, we have tended to crave authority, any authority, without ever questioning its claims. We have been more than willing to accept the linguistic dicta of those who have managed to set themselves up as experts — lexicographers, writers of handbooks, teachers, radio performers, and even newspaper columnists.

HALL'S BOOK presents an attitude towards language which has been that of linguists for a good many years, though it has never been acceptable to those who prefer the authority of prescriptionism to that of scientific evidence. Because the world is so full of a number of people who feel themselves competent to make *ex cathedra* pronouncements on language, the lot of the linguist has been hard. Though he does not presume to instruct engineers and chiropractors in the arcana of their respective crafts, he is continually subjected to the *ipsi dixerunt* of engineers, chiropractors, and others equally prestigious in the 'American way of life.' Assertions about language emanating from the murky realm of lay linguistics are not likely to be supplanted by the facts which Hall presents, for people often cherish misconceptions about language even more dearly than they cherish other misconceptions. Assuming that those who 'because they speak, fancy they can speak about speech' ever get around to reading Hall's book, it is not likely that they will approve of it, for untrained observers are chockful of notions about language, and tenacious of those notions.

In its style, *Leave Your Language Alone!,* as its very title would indicate, bears little resemblance to Hall's other books and articles, which are addressed to a learned audience. Some learned readers may not care for the rather familiar tone of the book; many will object to Hall's coinage *linguistician* for *linguist;* a few will take pride in spotting piddling errors, inevitable in any sustained product of human endeavor, such as Hall's attributing the ability to distinguish 120 vowel sounds in English to Daniel Jones instead of to the late Henry Sweet, prototype of Shaw's Professor Higgins (ah, there!). But *Leave Your Language Alone!* is the answer to those who, lacking linguistic training, wish honestly and sincerely to know something of the methods of linguistic study and to use that knowledge in arriving at their own linguistic decisions. Hall takes the reader rapidly and pleasantly through phonemics, morphology, syntax, meaning, linguistic geography, and historical grammar. He does with a great deal of success what he set out to do — no mean accomplishment indeed. It is a great pity that more linguists have not taken the trouble to popularize their science. Hall has shown that it is possible to do so without sacrifice of either quality or accuracy.

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EVALUATIVE SYSTEMS AT WORK

Nature Still, but Nature Methodized

FIGURE on a New Figure! If you want to reapportion the measurements of your bustline, you will be interested in the startling wonderful innovation in bust pads called REAL-E-YOU. They are not Falsies! Just tack them into your brassiere, wear them with play clothes or strapless gowns. They're priced just $2.00 pair. (An advertisement for Bon Marche department store.)

Seattle *Post-Intelligencer*

Keeping Up with the News

REDS STIR CHINESE WITH JAP BOGYMAN. HATED FOE TO MARCH AGAIN UNDER U. S. FLAG, PEOPLE TOLD. WASHINGTON. In their campaign to whip up sentiment for Korean intervention, the Chinese Communist have dug up an old but potent bogyman — the Jap. Peiping propagandists have gone into a Mammy Yokum spin to conjure up visions of the Japanese marching again under American auspices. Their press and radio has been filled in recent days with this propaganda line.

Chicago *Daily News,* November 7, 1950.

JAP 'POLICE' ARMY TRAINED TO FIGHT AGAINST REDS. U. S. REARMS 75,000 RESERVES MODELED AFTER AMERICAN TROOPS. TOKYO. Japan is quietly preparing for war under American leadership. Five years after its surrender this country of more than 80 million people is being rearmed by its conqueror to meet the challenge of Communist expansion in Asia. The nucleus of the new Japanese army — officially known as the National Police Reserve — was created by Gen. MacArthur four months ago after the outbreak of the Korea war.

Chicago *Daily News,* November 8, 1950.

Science and the Goals of Man

HERE'S a choice tidbit of information we're passing on to the Kefauver committee on how the 'boys' are now attempting to dope race horses. This modern, up-to-date method involves the use of — honest Injun — physiology in an effort to beat the saliva test. Remember the classical experiment conducted by the Russian physiologist, Ivan Pavlov, on 'conditioned reflex.'

Using Pavlov's findings as a base, the 'boys' tried a similar experiment on horses. During the training period, the horse would be given a shot of dope —

adrenalin, benzedrine or some other stimulant. This would make the horse very happy, indeed. No dope would be injected 48 hours before the race; but just before post time, a shot of plain water would be injected. And the horse, conditioned to the exhilaration of the previous shots of dope, would react accordingly!

Kup's Column, Chicago *Sun-Times*

Retort Casual

MRS. DOROTHY WHITE, platinum blonde wife of Oakland Attorney C. Wadsworth White, yesterday gave her own version of their marital woes and the current struggle for custody of their two minor children. As a matter of fact, she announced, the divorce action itself came as a big surprise 'and a complete shock because my husband and I were getting along wonderfully.' . . . White's charge, in which he said his wife threatened 'electrocution' of their youngsters, Cynthia, 7 and Lee, 3, brought this explanation from Mrs. White: 'Of course we had arguments. But this day, I wanted the car, and he wouldn't let me have it. So I just said that to him, casually, not as threat. Why the thought of such a thing is preposterous!'

San Francisco *Examiner*.

Eye of the Beholder

GILA MONSTERS are among the ugliest creatures alive. If the creatures were endowed with man's sensitivity to beauty and ugliness, it seems safe to say that the males and females would never be able to overcome their mutual repugnance, and the genus would simply die out.

'Nature Ramblings' in *Science News Letter*

Divine Discontent

AS CHAIRMAN of Allied Stores, Earl Puckett is more interested in figures (statistics) than in figures (Milady's contours). A fortnight ago, however, Puckett mixed finance and fashion for the edification of New York's fashion Group, a lunch-and-listen-organization. 'Fashion,' propounded Puckett, 'represents an important segment of the American standard of living . . . we must accelerate obsolescence, make women unhappy with what they have in the way of apparel, make them so unhappy that their husbands can find no happiness or peace in their excessive savings. Every year, every season must have its New Look. . .'

'Consumers,' added Allied's analyst, 'have the money but prefer to keep it than to exchange it for what we have been offering. . . With a sufficient amount of customer pushing, we may . . . obtain the part of spendable income that is now represented by excessive savings . . . Our soft goods fiber must be hardened.'

Forbes: Magazine of Business

☆ NEWS AND MISCELLANY ☆

NOTICE

ON INVITATION of University College of the University of Chicago, a CONFERENCE ON GENERAL SEMANTICS will be held at the University of Chicago, Friday and Saturday, June 22 and 23, 1951. Information as to programs and arrangements will be sent to members of the Society by mail and announced through *ETC.*

Individuals wishing to attend the Conference (at which time there will also be held the first membership meeting of the International Society for General Semantics) or to present papers are requested to read carefully the explanations given below.

Rules Governing Presentation of Papers

RULES FOR the selection of papers to be presented at the Conference on General Semantics have been approved by the Program Committee (appointed by the Governing Board of the Society) and by the Advisory Committee of University of Chicago faculty members (appointed by DEAN HARLAN M. BLAKE of University College). They are as follows:

1. All papers will be presented in a General Session. It is the opinion of the Committee that an interdisciplinary subject like general semantics should be presented before an interdisciplinary audience, and for this reason there will be no special sections for specialized topics.

2. In order to allow abundant time for discussion and to maintain a high standard, the number of papers to be presented will be limited.

3. Except for official addresses (presidential speech, reports of committee chairmen, etc.) there will be no papers by special invitation. Everyone receiving this notice (i.e. members of the International Society for General Semantics, members of the Institute of General Semantics, and readers of *ETC.*) will have an equal opportunity to present a paper. Papers are to be submitted anonymously. The author's name should be enclosed in a *blank, sealed envelope* attached to the first page of the typescript. The sealed envelopes will be opened only *after* the Committee decides whether or not the paper is accepted for presentation at the Conference.

4. Papers, or abstracts thereof, around 500 words in length, may be submitted at once. They should be sent to the Chairman of the Program Committee:

DR. ANATOL RAPOPORT
Committee on Mathematical Biology
University of Chicago
Chicago 37, Illinois

Papers and abstracts must be postmarked not later than April 15, 1951.

5. All persons submitting papers are requested to grant to *ETC.: A Review of General Semantics* first option on publication. It is expected that some papers, not especially suitable for the platform, will be found better suited for presentation in print.

During the two days of the Conference, there will also be held the first membership meeting of the International Society for General Semantics. Information about registration, lodging, travel, and program arrangements will be sent to all concerned as plans mature.

RUSSELL MEYERS, M.D., President
International Society for General Semantics

Explanation of Conference Rules

THERE ARE many individuals in the Society who, because of the newness of their interest in general semantics or because of lack of academic rank or professional connections, may feel hesitant about offering papers. The rule of anonymity is to underline the Program Committee's intention of examining each paper on its merits, and on its merits alone. The adoption of this rule is frankly an experiment; it is an attempt to take into consideration the wide range of interests and attainments, both academic and nonacademic, represented among those interested in general semantics.

There are also those who, having made notable contributions to general semantics literature or activities, may feel it strange that they are not specially invited to be on the program. The Committee apologizes to these for foregoing the customary courtesies in this respect; the omission is simply for the purpose of ensuring widespread participation. The Committee also feels that by selecting papers on the basis of the papers themselves (or abstracts thereof) rather than on the basis of the authors' names, it will be acting more in accord with principles of general semantics than would otherwise be the case.

Nonacademic members with ideas to submit are especially urged not to permit a lack of acquaintance with academic formalities deter them from submitting abstracts. An 'abstract' may be in the form of an informal (but of course informative) letter to the Chairman of the Program Committee.

In addition to the presentation of papers, the Conference is planned to afford abundant opportunity for discussion and informal conversation. This will be arranged for in the program itself, as well as in the entertainment, banquet, and social arrangements, which are in the hands of the Chicago Chapter of the Society.

THE FOLLOWING constitute the Program Committee: ANATOL RAPOPORT, chairman; IRVING J. LEE (Northwestern); WENDELL JOHNSON (Iowa); RUSSELL MEYERS, M.D., *ex officio* (Iowa).

University of Chicago faculty members who constitute the Advisory Committee are: HARLAN M. BLAKE, chairman (University College); THOMAS GORDON (psychology); S. I. HAYAKAWA (University College); BERT HOSELITZ (social science); CHARLES MORRIS (philosophy); ANATOL RAPOPORT (mathematical biology); N. RASHEVSKY (mathematical biology); DAVID RIESMAN (social science); JULIAN TOBIAS (physiology); HUGH WALPOLE (board of examiners).

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Foreword by S. I. HAYAKAWA

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CONFERENCE ON GENERAL SEMANTICS

at the University of Chicago

FRIDAY and SATURDAY, JUNE 22 and 23

All persons interested in general semantics and allied disciplines are cordially invited to attend.

HARLAN M. BLAKE
Dean, University College

RUSSELL MEYERS, M.D.
President, International Society for General Semantics

ADVISORY COMMITTEE FOR THE UNIVERSITY OF CHICAGO: Harlan M. Blake, *chairman;* Thomas Gordon (psychology); S. I. Hayakawa (University College); Bert Hoselitz (social science); Charles Morris (philosophy); Anatol Rapoport (mathematical biology); Nicolas Rashevsky (mathematical biology); David Riesman (social science); Julian Tobias (physiology); Hugh Walpole (board of examiners).

PROGRAM COMMITTEE: Anatol Rapoport, *chairman;* Irving J. Lee (Northwestern); Wendell Johnson (Iowa); Russell Meyers, M.D. (Iowa).

RULES for the selection and presentation of papers appear on pp. 159-160 of this issue of *ETC.* Information about program, lodging, travel, etc. may be obtained from

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